

paine (m)

*What the world says of
M. Paine.*

A R E P L Y

Paine M.
TO AN ATTACK

BY

HENRY I. BOWDITCH, M.D.

UPON THE ESSAY

ON THE PRINCIPAL WRITINGS OF P. CH. A. LOUIS, M.D.

AS CONTAINED IN THE

MEDICAL AND PHYSIOLOGICAL COMMENTARIES,

BY

THE AUTHOR.

298375
Washington
Male cuncta ministrat Impetus.—*Proverb.*

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THE
HISTORY
OF
THE
CITY
OF
NEW
YORK
FROM
1624
TO
1898
BY
JOHN
B. HOGAN
NEW
YORK
1898

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DR. PAINE'S REPLY TO "H. I. B."

To the Editor of the Boston Medical and Surgical Journal.

SIR,—You will much oblige me by publishing the following reply to the attack upon my Essay on the Principal Writings of P. Ch. A. Louis, M.D., which appeared in your Journal on the 9th, 16th and 23d of September, purporting to have been written by "H. I. B." I have divided my communication into several parts, so that it may interfere as little as possible with the rights of other correspondents.

New York, Oct. 4th, 1840.

Very respectfully yours,

THE AUTHOR OF THE
MEDICAL AND PHYSIOLOGICAL COMMENTARIES.

I am sensible that it is a good rule for an author to leave his work to the justice of the world, which never fails to discover its motives and its merits, whilst it holds in the same balance those of his critics. But, it has been the destiny of my book to have met with an unusual adversary before it can have had the advantage of any general perusal; whose purpose has been to impede its circulation by misrepresenting specifically my Essay on the "Principal Writings of M. Louis," and the objects of the work in a general sense. I am, therefore, induced to depart from my views of expediency under ordinary circumstances, and to protect myself against the misstatements of "H. I. B.," till the public may take the work into its own charge.

Having, then, undertaken my own defence, I shall notice every point of attack, *in extenso*, that the work may be neither imperfectly done, nor my neglect of any objection be construed into an admission of its correctness. But, more than myself, I have in view a defence of the Essay which is the special subject of assault, since it relates to some momentous questions in medical science.

It is painful to me in performing this act of justice, that I am compelled to touch upon the morals of another, or to employ any language of recrimination. It is more especially so to recur to a task which I assumed in relation to the writings of M. Louis, and which was undertaken solely in behalf of my profession, and of that general experience of the world upon which my work is founded. But, having determined not to remain silent, I must be efficient. My whole position in respect to M. Louis being susceptible of entire defence, it must not be neglected from any indisposition to renew the subject. I will endeavor, however,

to avoid the expedient of my critic in substituting fraud and personalities for fact and argument. It was doubtless thought that the former would deter me from any collision with him; and here my assailant would have calculated rightly, had he not overstepped the mark as to my honor and veracity.

It may be assumed that "H. I. B." has made the best of his cause, not only for an important reason yet to be assigned, but from his declaration that "we are devoted lovers of the plan originally proposed by others, but first fully developed by Louis, viz., the Numerical Method. We hereby give in our faith," &c. Should it appear, therefore, that "H. I. B.," actuated, also, by the lowest motives, has utterly failed in all his points of attack, the work which he assails will be fairly entitled to the consideration of having placed certain dangerous innovations upon philosophical and practical medicine in such an aspect as to defy their most interested advocates. The public, therefore, will perceive that it is deeply interested in this matter; and in proportion to the intrinsic importance of a subject, is ever its disposition to sustain the advocate of truth,—and especially so, where it is apparent that he has had at heart the well being, alone, of his fellow man. But, I ask for no sympathies. My defence looks only to simple justice, and at questions in which the medical public is as much interested as myself. It is therefore especially upon this principle that I expect a patient hearing, and an impartial verdict. If that verdict be my exoneration, it will then appear that I have been hitherto employed in endeavoring to advocate the cause of truth, the violated dignity of the profession, and the desecrated worth of its illustrious founders and of those devoted men who have carried the science of medicine to its present high advancement. In the Essay in question I have endeavored to turn aside the scorpion lash, and here, as elsewhere, to rescue medicine from its imputed "infancy," and to place it where it belongs, in virtue of its wide range of observation and surmounted difficulties, at the head of all human pursuits. This is its rank,—not because of its abstract importance to the necessities and the happiness of mankind, but because no other science was so early cultivated with zeal; no other whose foundation was originally laid in the Baconian philosophy; no other to which that philosophy has been almost uninterruptedly applied for more than two thousand years; no other upon which the efforts of genius have been so largely and powerfully concentrated; no other which swallows up all other science; no other which has left so little for the labors of posterity. I speak of its proper attributes,—not as they are illustrated by ignorance or speculation, or as they are misrepresented by ambition or honest credulity.

I shall have little to say of the style and temper of my reviewer. They carry their own elements of interpretation; and, whilst they seem to denote an unexpected calamity, they have probably rendered sufficiently suspicious the pretences which they were designed to enforce. I come, therefore, at once, to the subject matter of *complaint*. In the first place, however, I would say that, "H. I. B.," the author of the strictures under examination, having impugned my veracity, should have had the magnanimity to have written without disguise. The ini-

tial signature, although sufficiently indicative to many of the authorship, is concealment to most, and may be soon unknown. It serves, however, as a pretext for open avowal should the strictures remain unnoticed, and as a loop-hole of escape should a merited exposure be made. I shall therefore place the authorship upon a ground that shall be intelligible to all, and that no innocent man may be unjustly suspected. I assume, then, that "H. I. B." is Doctor Henry I. Bowditch, the Translator of Louis on Typhoid Fever, who also "revised and altered" Cowan's translation of Louis on Phthisis, and who is or was the copy-right proprietor of the American editions of those works. The ground of my conclusion as to the identity of the writer is the initial signature to the review, its appropriation by Dr. Bowditch in his notes to the foregoing works, its remaining uncontradicted, its undoubted import by others, and the internal proofs which abound in the article.

Dr. B. professedly undertakes the defence of M. Louis. After saying that he has "no fears for the foundation of Louis's main ideas,"* he continues,—“Still we feel unwilling to allow any writer, under the pretence of examining the philosophical views of M. Louis, to traduce his character. If all that is said by Dr. Paine of the former physician of La Pitié be true, M. Louis deserves to be treated not merely as one wholly unworthy of confidence in medicine, but as an individual of a base private character.”—(P. 73.) And farther on he repeats, “Dr. Paine attempts to prove Louis a liar (we are aware of the meaning of the word), by quoting a translation.”—(P. 80.) Here, then, Dr. B. has unnecessarily magnified the inductions which I had drawn, and perverted my Essay to the injury of his friend, since I certainly neither expressed nor entertained such an opinion; and the best evidence which I can offer of my reliance upon his veracity is to be found in the unhesitating manner in which I have received all his statements. This confidence in his veracity appears, also, repeatedly in other Essays, where I have ever quoted his facts and opinions as substantial authority. (See Vol. 1, pp. 314, 408, 533; Vol. 2, pp. 320, 626, 629, 632, &c.) And when employed in repelling his imputation of duplicity and falsehood to venerated members of the profession, I have not retorted the allegation, except in a suppositious manner to show the unhappy results of discrediting medical testimony. (See Vol. 1, pp. 295, 331—332; Vol. 2, p. 680—681.) We have constant examples before us, and around us, where ambition so far saps the moral principle as to render it careless of public rights; but where, also, it is restrained in respect to those private virtues which form the important cement of society. Shall we, for instance, pronounce the politician a “liar,” and of “a base private character,” because he disregards the public weal to carry the objects of party, or even to advance the aim of selfish ambition? It is exactly in the foregoing respect that I have considered M. Louis,—publicly wrong, but probably amiable in private life. This is one of

* The introductory part of Dr. B.'s attack is a common reëcho of what appears in Cowan's Preface to the work on Phthisis, in relation to Broussais's *Examen*. “Whilst thus deprecating,” says Cowan, “the tone of prejudiced and illiberal feeling which pervades the strictures of M. B——, we are not anxious about their effects on the volume before us,” &c. The notice, however, is respectful, notwithstanding Broussais “has alternately impeached the accuracy and veracity of M. Louis.”—(P. 59.)

the paradoxes of human nature, which could not be inferred *à priori*, but which is revealed through daily experience. However unhappy this system of morals, the broad limits exist as I have represented them; though it be certainly true that the barrier is easily passed, and the distinction obliterated.

What I have said of M. Louis's ambition as a public man, of his diplomatic expedients, of the rights he has invaded, of the characters he has assailed, of the degradation to which he would reduce the profession, is predicated of premises which are before the world, in the language of my author. That world, and not an interested partizan, must be my judge. And now, to do the fullest justice to all, I may say that one of my author's judicious friends and defenders, in a late notice of my Essay, does not intimate that I had questioned his veracity, and allows my "general fairness and candor."*

But, what is most extraordinary, is the affirmation, that, "Dr. Paine attempts to prove Louis a liar *by quoting a translation*; whereas, if he had taken the trouble to look at the original, he would have found that he by no means could prove what he wished."—(P. 80.) Now Dr. Bowditch being the author of that translation must certainly take the imputed blame to himself. Least of all should he put forth his own version as the ground of his author's error, and by thus repudiating it, endeavor to show that through his own inaccuracy he had led a confiding man to do an act of the highest injustice to another, and then turn upon him as a calumniator. It will be seen, also, that this matter relates to a question in which M. Louis has taken no part, and is one with which his veracity has not the slightest connection. That nothing, therefore, is found by Dr. B. to justify his imputation is manifest from its guarded manner, the nature of the premises, and the subject matter which will be soon set forth.

I had certainly some suspicion that Dr. B. would complain of any version I might have attempted of the writings of M. Louis,—especially those on Phthisis and the Typhoid Fever. I therefore took the precaution of adopting his own translations; and this more especially as we are assured by Dr. Bowditch in his Preface to the latter, that he believed he had "*translated truly the meaning of the author in every respect*," whilst that on Phthisis had been "*revised and altered*" by the same hand, and duly registered in the archives of the nation. Having thus determined upon the *safest* course, and stated the fact at page 686, and no objection having been raised against the translation in Europe or America, till now made by the translator himself, it is clearly absurd to suppose that it was incumbent on me to have compared it with the original work, since this would have involved the necessity (on account of the multiplicity of my extracts), of comparing the whole of the four volumes on the typhoid affection. My objector says, "If he had taken the trouble," &c. The task is more difficult than the requisition to perform it, since the principle exacts not one, but an universal comparison. And suppose that others, as Müller, for instance, whom I have quoted largely and critically, should start the same objection; will it be

* New York Journal of Medicine and Surgery, No. 6, Oct. 1840, p. 423.

said that it was my duty to have compared the whole of Baly's translation of 850 compact pages with the original German; and especially when I stated in my work that I should adopt acknowledged translations of foreign authors in preference to any version of my own, that no one might complain of misconstruction. Dr. B., however, having condemned his own translation, renders it proper to inquire how far he may escape under this subterfuge, and especially whether he has made out his principal, but more concealed object.

The reader, by referring to the connection of the foregoing quotation with other remarks by Dr. B., will observe that the imputed attempt to convict M. Louis of "lying" is predicated of the proof by which I thought it expedient to identify M. Louis with the superintendence of the typhoid cases. Since, however, M. Louis nowhere intimates that he had not the entire charge of these cases (the only ones which I have assigned him), but, on the contrary, everywhere implies that such was the fact, the charge upon which I am arraigned is necessarily a fabrication, and as criminally injurious to M. Louis as unjust to myself. What, then, was the object of Dr. B. in this experiment? Could there have been any other than that of forcibly implying that M. Louis had denied his care of the typhoid patients and that I had endeavored to prove it otherwise? But, a greater consequence is involved, viz., that of impressing the reader with the certainty that M. Louis was not the prescribing man. The question, then, had not the most remote connection with my author's veracity. So unsuspecting, indeed, was I of "attempting to prove M. Louis a liar," I had supposed that I was merely employed in receiving his own testimony that he was responsible for the treatment of the 138 typhoid cases, upon which he erects his generalizations in relation to the typhoid fever. Dr. B. professes not to understand my motives even for this. I will tell him, what the reader, however, who may have seen my Essay, sufficiently comprehends. About the time my work was going to press, in a conversation with one of M. Louis's able disciples, I told him that I considered Louis not only vulnerable as a pathologist, but in his treatment and therapeutical conclusions. The defence was immediately set up, that Chomel was the hospital physician, and consequently alone responsible. I had little doubt, therefore, that Dr. B. would be very likely to resort to the same pretext. Indeed, I have stated this very fact in my Essay (p. 800). "*Hinc illæ lachrymæ.*" In every aspect of the case, however, it should be the pride of his followers to admit, what Louis constantly implies, that he had the active superintendence of the 138 cases, since his "rigorous conclusions," anatomical, pathological, and therapeutical, would be, *prima facie*, seriously impaired without this evidence of their practical foundation.

Still, it is manifest that Dr. B., for divers reasons, is strongly disposed to avail himself of the pretext; by which the reader will perceive that I had not been greatly mistaken in my conclusion, nor unwise in my precautions. Thus, in connection with the quotation which leaves me impeaching the veracity of M. Louis, Dr. B., to give it plausibility, affirms that "there is no such expression in the work on typhoid fever,

as 'I prescribed,' meaning thereby to state definitely that Louis prescribed." It is here that Dr. B. disavows his own translation of an instance which I had cited to the foregoing effect, and one of a very comprehensive nature,—namely, "*we abstained* from bloodletting in the others, either on account of," &c.,—the original being, in this instance, "*On abstint*," &c. Again, a little farther on (p. 448), M. Louis says, or his translator for him,

"We allow generally that a moderate delirium does not require a special and very energetic treatment, and according to the most common practice, *we confined ourselves*, in order to counteract it, to the application of blisters and sinapisms to the lower extremities, in the patients *whose histories* I have compiled."

I have also taken this example on account of its comprehensive import, and have confined myself to Dr. B.'s translation, not only because I originally proceeded upon that principle as the one which I supposed would be most acceptable, but to show that, if the version be not exactly literal, it was rendered otherwise from the conviction of Dr. B. that Louis was the "acting man" in relation to "the patients whose histories he had compiled,"—whilst we have the doctor's affirmation that, in his belief, he "had translated *truly the meaning* of the author in every respect."—(*Preface.*)

I am now no farther interested in this question than in carrying out my intention of simply performing an act of justice to Chomel. If this gentleman chooses to take the credit of the 138 typhoid patients,—be it so. The principle, as to the only object of my analysis of the treatment, will be in no respect affected, unless it be to diminish still more the value of my author's therapeutical generalizations. But, till then, I must insist upon the evidence for my conclusion, that the treatment was not only the unqualified basis for doctrines of the highest moment, but was projected by M. Louis himself. This, indeed, is everywhere so apparent from the manner in which M. Louis identifies himself with the history of every case from the time of its admission till its exit, that the fact cannot be doubted till contradicted; and this will never be done by M. Louis, or by any considerate friend, not only for the reason already stated, but as it would imply an attempt to have misled the profession. Take, again, the following examples of the manner in which the translator "*truly*" understood "*the meaning*" of his author. Thus,—"*Our patient had kept his bed*," &c.—(Vol. 1, p. 4.) "*Our patient was placed*," &c.—(P. 6.) "*Although our patient was placed*," &c.—(P. 68.) The work abounds with expressions which appear to denote that Chomel extended his kindness so far to M. Louis as to suffer him to institute his experiments upon these 138 patients. The reader may find the proof on almost every page. I referred specifically to a multitude of instances. Vol. 1st of the translation may be consulted at pages 21, 45, 64, 82, 85, 87, 95, 121, 132, 148, 159, 237, 244, 263, 343; and vol. 2d, at pp. 7, 14, 27, 29, 30, 39, 45, 60, 74, 77, 128, 129, 136, 152, 162, 171, 180, 187, 193, 197, 198, 200, 202, 205, 206, 207, 208, 234, 261, 274, 290, 291, 307, 342, 343, 382, 384, 391, 438, 439, &c. The foregoing references relate gene-

rally to instances which show an intimate connection of the observer with the prescriptions which are announced. I will cite one for the purpose of showing also the *means* of information upon which M. Louis has founded his hypothesis as to the *primary* seat of the typhoid affection in a lesion of the glands of Peyer. After detailing the seven days' symptoms with which the patient had been affected before entering the hospital, M. Louis continues,—“The *patient* assured *me* that he had had a little *delirium*.” The same day we read “slight stupor,” “*memory seemed* to return to the patient as *I questioned* him,” &c. Then follows immediately the prescription. (Vol. 1, p. 342.) (*My Italics.*) No one can take up the work on Typhoid Fever without feeling irresistibly that M. Louis had the active superintendence of the cases which he describes. I cannot find Chomel alluded to beyond the “Advertisement,” except in three instances, and then only very incidentally. Nevertheless, I was indifferent to the fact, except so far as I felt disposed to silence prevarication, as will appear from my declaration that,—“this would be *unimportant*, since our author makes the cases entirely his own, erects generalizations upon them, and defends the whole treatment.”—(P. 685.) This construction, however, Dr. B. studiously conceals. But his objects, and mine, are now before the reader. My comments upon the treatment, from which I was disposed to separate Chomel, never would have been made in relation to the practice of any man who had not treated the profession as discourteously as I have shown of M. Louis; and had this distinguished individual been less unceremonious, my Essay would either have had no existence, or would have appeared in a different aspect. What I have set forth of his hostility to the profession is urged as a merit by Dr. B. when he says, “Louis, *disgusted* with the uncertainty prevailing *in all* branches of medicine,” &c.—(P. 79.) (*My Italics.*) This shows that my strictures were wanted; and if the profession do not find them acceptable, I can only regret that I had attempted so ungracious a service.

The reader will have seen that it was the apparent object of Dr. B. to pervert the foregoing subject so as to extort the charge that I had attempted to “prove M. Louis a liar.” So far it was designed to impute to me what I was not attempting; but, a greater object doubtless was to establish the impression more fully, that M. Louis is not responsible for the treatment of the typhoid cases. The secret of this will be found in my comments, and in the death of more than one third of the patients. The solicitude of Dr. B. upon this question may be seen in his hunting the work through to ascertain that there is no such expression in the original as “I prescribed.” The whole state of Dr. B.'s mental operations will be farther disclosed by my next exploration.

In connection with the foregoing subject, it is said by Dr. B. that “Chomel, notwithstanding Dr. Paine takes it upon himself to declare to the contrary, is *always the chief physician*.”—(P. 79.) This is not only a positive averment, to convict me of untruth, but is intended to carry out the impression, as implied in the statement already examined, that I had attributed the superintendence of the hospital patients, in a general sense, to M. Louis. Should it, however, turn out in

all respects exactly the reverse, Dr. B. must occupy a worse position than he had provided for myself. In the first place, then, I have carefully restricted M. Louis's agency to the 138 typhoid patients; and, secondly, I have stated the very declaration of M. Louis, upon which Dr. B. brings his charge that "Dr. Paine takes it upon himself to declare to the contrary." Thus,—“It should be observed, *in the first place*, that in the ‘Advertisement’ to the work, Chomel is spoken of as having *the supervision of the hospital*.”—(P. 685.) Now, what says the ‘Advertisement’? “At that time under the superintendence of M. Chomel;” or, as the translator has it in his new version (and as will appear more fully hereafter), “under the *supervision of* Mons. Chomel.” This is every word that appears upon the subject, and, as will be seen by the entire extract in my second number, is noticed by M. Louis in a transient manner. This well-known fact I have no where questioned, and it was stated by me for the very purpose of contradistinguishing the 138 typhoid patients from the general mass, which I had never doubted were under Chomel's exclusive superintendence. And here it is worthy of remark, that Dr. B., in the very midst of this serpentine course, not only allows more than I had contended for, or believed to be true, but contradicts the whole of his statement, in saying that M. Louis “gave up his business, and entered as *clinical aid* to his friend Chomel.” Again, the same thing is implied in the following sentence,—“but it remains for Dr. Paine to find out that Chomel ought to be ashamed of his *co-laborer*,” &c.—(Pp. 76, 79.)

Finally, the work on Typhoid Fever was designed to unsettle the most important principles in physiology, pathology, and therapeutics; and, not being otherwise stated, it is inferable, *à priori*, that this vast enterprise had the advantage of the fundamental requisite,—that is to say, the author's personal superintendence of the 138 cases of the typhoid disease. I need not inform my reader, that it is not unusual in French hospitals for the chief physician to delegate a certain class of patients to other physicians for the work of experiment. This fact, and its consequences, I had occasion to illustrate, by example, in my “Commentaries.”—(See Vol. 1, p. 305.) I may say, however, that, as in my Essay on the writings of M. Louis, I had in view no partial objects, but principles co-extensive with the importance of the healing art.

I have dwelt long, perhaps tediously, upon the foregoing implications; but they had been sedulously involved, and put forth with a commanding confidence. A full analysis has been therefore indispensable, not only to develop the motives and deportment of Dr. B., but to place my relations to M. Louis, as a commentator, in their proper aspect. Nor can I yet conclude, without admonishing the reader that something yet remains to illustrate the *quo animo* of my critic in respect to the present subject, and by which it will appear that, through a fraudulent translation and surreptitious quotations, designed to pervert the truth in other respects, he simultaneously enforces the belief that M. Louis was not only intimately concerned with all the cases of every disease to which he refers in his work on the typhoid affection, but, in so doing, imputes to that work a foundation which has no apparent existence. The state-

ments and allegations are ingeniously conceived, and artfully involved. They require, therefore, a more or less elaborate examination. There is no one which is not essentially destitute of truth, and they are generally distinguished for a combined purpose of impugning my honesty or veracity, of misrepresenting my work, and of misleading the profession as to M. Louis's labors and doctrines. It is true, that in certain instances I am allowed the alternative of having read my author carelessly or not at all. But this is no part of the character of my work; and, whilst I am not ambitious of the indulgence, its sincerity is contradicted by the ample evidence in my Essay that I had not only read my author repeatedly, but with careful attention. The critical press, also, which has been thus stealthily invaded, requires redress; and I cannot but think, therefore, where so many wrongs are committed, the public must feel an interest in their proper exposure. I lament the necessity of employing any language that may be offensive to a chastened taste; but, vulgarity may not always be rebuked by forbearance, and there are transgressions which are better described by their right names, than by any circumlocution.

No. 2.

I SHALL now take up the examination of a subject to which I referred at the close of my remarks in the preceding number. The charge which is conveyed, and the manner of its execution, being designed as the most fatal stab at my integrity and my work, the reader must bear with me whilst I place upon record one of the most remarkable instances of critical chicanery that has probably ever fallen under his observation. I could expose the fraud, and thus cover the whole of Dr. B.'s article with confusion, in a few words. "When a man," says Dr. B., "proves false in one position, we suspect him in regard to others" (p. 107); or, as Quintilian has it, "*Maledicus a malefico non distat nisi occasione.*" But it is due to the profession, which is deeply interested in subjects of such importance as are involved in the writings of M. Louis, that I should show, unequivocally, that I have treated them with the most scrupulous fairness. It is due to men of science, and of laborious intellectual industry, that I should make a full representation of the fraud which is here practised, that the pen of interested or unprincipled critics may be less frequently envenomed, and that I should erect a gibbet at which envy itself may stand appalled. It is due to M. Louis, and to the readers of his works, that I should fully expose the manner in which the translator perverts them to interested purposes. It is due to truth, to my honor, and to my work, that I should analyze throughout an artful contrivance by which Dr. Bowditch attempts a plot of such comprehensive design. The specifications which I shall establish are,—

1st. A perversion of a pretended and fundamental extract from my Essay, in disconnecting a part of a sentence from its indispensable relation to words immediately preceding.

2nd. An erasure from the body of the extract of the two most important words, by which the whole meaning is altered.

3rd. That, by this act it is endeavored to be shown, in part, that I have fundamentally misrepresented M. Louis, and, therefore, that my Essay, if not my whole work, is radically defective.

4th. That this intended inference from a surreptitious quotation is directly enforced by an accompanying declaration as set forth in my third specification, and that this charge is also of a general nature.

5th. That a passage of specific import, and relating *exclusively* to the ostensible ground of the supposed anatomical lesion of the glands of Peyer, is put forth in a mutilated form, to show that I had given "an unfair impression of his (M. Louis's) labors;" when, in its appropriate place, I had allowed more than is required by M. Louis, or by Dr. B. himself.

6th. That the offence which Dr. B. was thus imputing to me, was perpetrated in the very act and in an unexampled manner by the projector.

7th. That the charge is sustained by a false translation of the original French; and that, in thus falsifying the author, an impression is attempted that I had committed the crime.

8th. That the translation, in its most essential attribute, is wholly different from the original translation by the same hand, which had declared in its Preface the belief that it had "translated *truly the meaning* of the author in *every* respect."

9th. That the perverted translation ascribes an importance to the supposed characteristic anatomical lesion of the typhoid affection, which is not even insinuated by M. Louis.

10th. That the perversion of the translation, and the induction from it by Dr. B., appropriate a foundation, in a general sense, to the work on Typhoid Fever, which M. Louis never designed in the sense which is thus conveyed.

11th. That Dr. B. employs a mutilated fundamental quotation, and a false translation, as a principal means of convicting me of falsehood, when the true quotation and the true translation show, respectively, that I have stated the truth.

12th. That, contrary to his usual practice in the review, Dr. B. suppresses the original French in the instance under consideration, and a reference to the page in my work.

13th. That, as a critical writer, Dr. B. has adulterated a most important element of the press, thrown suspicion over its honest guardians, and by thus blunting the edge of criticism he has opened wider the door for licentiousness in writing.

Dr. B. is speaking specifically of the supposed characteristic *anatomical lesion* of the typhoid affection.—(P. 82—83.) He goes on thus: "Dr. Paine may think he has gained his end, and that Chomel and we both allow that Louis generalized too quickly—and that in stating the

anatomical characteristic of the typhoid fever to be a lesion of Peyer's patches, we declare that the symptoms are dependent upon this change of these patches. Now let us examine Louis's works and see what he says upon the subject; and first, we must say that in stating the foundation of Louis's assertions, Dr. Paine, as usual, gives an unfair impression of his labors, and leads the reader to believe that all the cases of any disease that Louis examined in order to arrive at definite conclusions in reference to the characteristic lesion of the typhoid disease, were '50 cases of acute disease having certain other analogies, and 83 other cases where these analogies are said to have been more or less wanting.' " The latter clause of the sentence from "50" embraces my words, which stand in their proper relations, and bear the usual marks of a quotation.

Now I will show by the statement as it exists in my work, that I represented the whole truth, and nothing but the truth. The pretended quotation is at my page 686, and refers *exclusively* to the number of cases which are offered by M. Louis as the absolute foundation of his generalization of the *anatomical* lesion of the glands of Peyer. Thus:

"The first thing, we say, which excited our surprise, is the broad affirmation that a lesion of the glands of Peyer may be taken as the anatomical characteristic of typhoid fever, *because it was present in 50 cases of acute disease having certain other analogies, and was absent in 83 other cases where those analogies are said to have been more or less wanting.*"—In the same paragraph, and connected with the same subject, I use the words, "a comparatively *unimportant* lesion of structure;" that is, of Peyer's glands. The bearing of the word "*unimportant*" will be seen hereafter.

The foregoing 50 and 83 cases (133) make the whole number of dissections upon which M. Louis professes, in his work, to have founded his conclusion as to the glands of Peyer. The reader must not be drawn off by any considerations relating to the experience of M. Louis beyond what is the ostensible foundation of his work. With this alone I was concerned; though it will be ultimately seen that I allowed any latitude of experience that might extend beyond the work itself. I should add, however, that I have objected to the employment of cases which do not appear to have received that attention from M. Louis which was bestowed upon the 138 cases of the typhoid affection, or which are not analyzed according to his requisitions of other writers,—exempting, also, from objection the 83 anatomical inspections in other diseases. It is therefore a primary object with Dr. B. to make it appear that all the cases mentioned in M. Louis's "Advertisement" actually came under his critical observation.

But, to the point. What does M. Louis say of the basis of his conclusion as to the anatomical lesion of Peyer's glands? I will first state it as expressed in the conclusion of a paragraph, which I will render memorable by soon quoting in full. Thus:—"so that I have analyzed *the diseased changes* of the viscera of 133 subjects, and the *symptoms* of nearly 900."

Now as the *anatomical* lesion could only be ascertained by dissection,

the reader will see that I not only stated the entire number of cases which were the ostensible basis of the conclusion, but distinguished the *classes* of cases which make up the 133 anatomical inspections. This, as will be seen, was repeated in other places. I did it for the purpose of precision, and that the whole merits of the subject should be distinctly before my reader. But, as will be soon seen, Dr. B. endeavors to instil the belief that the whole 900 cases should be taken into the account as it respects *the anatomical* lesion of the glands of Peyer, and then charges me, directly, with having in my statement suppressed a part of the facts, or, rather, with having misrepresented them.

Let us now go back to the preceding quotations for the purpose of making out my specifications as to the extract from my Essay. The fraud consists in not taking along the words "*because it was present in,*" by which the 50 cases are shown to have been instances in which the anatomical lesion was found, and in expunging from the very body of the quotation, the important words "*was absent in.*" But, shall charity come to the aid of my critic, and shall he be indulged with the allowance that the quotation was carelessly made? The selected words are all arranged as they stand in my Essay, and are duly marked, whilst the deliberate mutilation of the extract will be fully shown by its artificial relation to the perverted extract from M. Louis.

The reader will therefore see, that only 133 cases can be brought to the anatomical lesion, and that I might thus silence my adversary at once by throwing his mutilated quotation back upon him. But, I have yet a long account to settle with him, and have chosen not only to expose the whole of this affair, but, in so doing, to show to the world with what scrupulous fairness I have treated M. Louis.

After the surreptitious quotation from my work, Dr. B. goes on thus: "After speaking of the state of doubt in which physicians were in reference to fever—some calling it a gastro-enteritis, others a putrid adynamic, ataxic and typhoid fever, Louis continues thus."

Having now arrived at the remaining ground of my specifications, I shall present the quotation as rendered by Dr. B. in his original translation (having then "*translated truly the meaning of the author in every respect*"), and the perverted version, in parallel columns; and then subjoin the original French which Dr. B. suppresses. The original punctuation is observed throughout.

Rejected Translation.

"I collected during this space of time, in addition to some incomplete facts, 138 observations of typhoid fever, 50 of which were relative to individuals who had died of it. I analyzed all, and in order to determine among the numerous lesions found in the patients who died of those that are peculiar to typhus, I compared them with the alterations found as consequences of other acute

"Revised and altered" Translation.

"During this period I obtained, with the exception of some imperfectly recorded facts, 138 observations of the typhoid fever, 50 of which related to individuals that died. I analyzed both, and in order to know, among the numerous lesions found in those who died, those that were peculiar to the typhoid affection, I compared them with the alterations observed in consequence

diseases, in 83 subjects whose histories *I learned*. *I did the same with the symptoms* observed in those affected with typhus fever, or with any other acute affection, which terminated in restoration to health or in death, so that I have analyzed the diseased changes of the viscera of 133 subjects, and the symptoms of nearly 900."

of other acute diseases, in 83 subjects, whose cases *I carefully recorded*. *I did the same when examining* the symptoms in patients affected with the typhoid disease or any other acute affection terminating fatally, or by return of health. So that in fact I have analyzed the alteration in the viscera of 133 subjects who died, and the symptoms of nearly 900."

The suppressed Original.

"J'ai rassemblé, dans cet espace de temps, à part quelques faits incomplets, cent trente-huit observations de fièvre typhoïde, dont cinquante relatives à des individus qui ont succombé. J'ai analysé les unes et les autres, et afin de connaître, parmi les nombreuses lésions de ceux qui ont péri, celles qui sont propres à l'affection typhoïde, je les ai comparées aux altérations observées à la suite d'autres maladies aiguës, chez quatre-vingt-trois sujets dont j'ai aussi *recueilli* l'histoire. *J'ai fait le même travail pour les symptômes*, chez les malades atteints de fièvre typhoïde ou de toute autre affection aiguë, terminée par le retour à la santé ou par la mort; en sorte, que j'ai analysé les altérations des viscères de cent-trente-trois sujets, et les symptômes de près de neuf cents."
—(P. ix.) *The Italics are mine.*

The reader will perceive that the old translation is the true one, whilst the new translation is false, and adapted to the mutilated quotation from my work. Doubtless, Dr. B., by altering the style of the whole, and by suppressing the French, expected to escape his own snare. It is also wrought with so good a device as to require some management to secure effectually the proper game. I shall, therefore; in the first place, point out one of the most important violations of the original, and then show its application. This violation consists in substituting "*I carefully recorded*" for "*I learned*,"—the original being "*j'ai recueilli*." As this will prove to be an important word, it may be well to satisfy all that my critic's old translation is right, by giving the various import of the word from Chambaud's large dictionary. Thus:—"Recueillir. To collect, to pick up, to compile, to gather." It will soon be seen, also, that Dr. B. understands its true meaning, as he had just previously rendered it, in the old translation, "*I collected*."

Now the reader must turn back to the mutilated and the true extract from my work, by which I was pointing out M. Louis's data for his generalization as to the supposed anatomical characteristic of the typhoid fever,—that is to say, "50" and "83" dissections. This statement of mine, therefore, has nothing to do with any other foundation that may constitute the general basis of M. Louis's work. That, as I have said, is stated in its appropriate place. But, Dr. B. takes this extract, mutilates it, and places it at the head of the foregoing mutilated extract from M. Louis, to sustain his accompanying charge that, "Dr. Paine, as usual, gives an unfair impression of his (Louis's) labors,

and leads the reader to believe that all the cases of any disease that Louis examined," &c.

The false translation of *j'ai recueilli*, or "*I carefully examined*," is designed for a triple purpose; the first of which is, that it should bear upon the mutilated extract from my work, and the imputation of Dr. B. that I had given by it a false account as to the number of cases employed by M. Louis in arriving at his generalization as to the anatomical lesion of the glands of Peyer; secondly, that I had also given by it "an unfair impression of his (Louis's) labors," in a general sense; and, thirdly, that the whole 900 cases are as much entitled to be regarded as a genuine foundation of the work on typhoid fever, as the 138 cases of that disease which M. Louis did probably *carefully record*, and whose treatment, there can be no doubt from his work, he conducted himself. It is this third bearing at which Dr. B. especially aims. Having conveyed the false impression, that I had suppressed all allusion to any other than "50 cases of acute disease having certain other analogies, and 85 other cases where those analogies are said to have been more or less wanting," he proceeds to alter the extract from M. Louis to show that the whole 900 cases were of equal value, and thus to enforce the more strongly the injustice he imputes to me. The work on the typhoid fever shows, on every page, the close attention which M. Louis bestowed upon the 138 cases of that disease; whilst we are only told, in the foregoing extract, and no where more specifically, that he "learned" or "collected" the residue of the 900 cases;—from which it is clear that he collected them from the *records of the hospital*. In any event, we are no where given to understand that he "*carefully recorded*" them himself, whilst the intent of the translator will become more glaring by his fabrication of words, and another false translation of *j'ai recueilli*, which will soon be stated. Nevertheless, I made no objection to the 83 dissections in which the lesion of the glands of Peyer was wanting, although we are only told by M. Louis that he "collected" their histories. I make this statement to show the bearing of the new translation, "in 83 subjects, whose histories *I carefully recorded*." This refers directly back to the 138 cases of typhoid fever which were the peculiar subjects of M. Louis's attention (according to the work), and, by the new words, it is implied that the 83 cases were not less the subjects of his care. The sentence then runs into the next following, so as to make it appear that M. Louis as *carefully* observed the symptoms of the whole 900 cases. Thus:—"in 83 subjects, whose cases *I carefully recorded*. *I did the same when examining the symptoms* in patients affected with the typhoid disease or any other acute affection terminating fatally, or by return of health." But, that my critic might make this conclusion the more certain, he adds another bait, and actually fabricates the words "*when examining*,"—whereby he makes M. Louis not only to have "*carefully recorded*" all the cases, but to have actually "*examined the symptoms*" of the 900 cases. Nor is this the worst of it; for, by interpolating the words "*when examining*," Dr. B. perfects the design at which he was aiming, namely, that of making out a fact, that M. Louis not only "*carefully recorded*" all the cases, but that he made the

record "when examining the symptoms" of the 900 cases. Thus :— "in 83 subjects, whose cases *I carefully recorded. I did the same when examining the symptoms,*" &c. The reader, by reverting to the extract, will see, also, that the words "when examining," in their relation to the preceding sentence, are even of more comprehensive import ; as they represent M. Louis actually *analyzing, comparing, &c., "when examining"* the symptoms of the 900 cases.

The reader will now see the connection of the whole, by referring to the old and new translations, and the mutilated extract from my work. I may say, however, that having thus *fixed* the whole matter, the Doctor proceeds to apply it after the following manner ; viz. "One would think that the *accurate examination* of about 1000 cases, and the autopsies of 1–10 of them, would have enabled any accurate observer to decide whether a lesion was *unimportant* or not." The word "*unimportant*" is in Italics, and refers to my use of it in relation to the anatomical lesion of Peyer's glands, in the paragraph from which the mutilated extract is taken. The reader will now see, therefore, the impression which is intended to be conveyed, that, in stating the number of 133 cases as the basis of induction as to the *anatomical* lesion of Peyer's glands, I had suppressed a part of my author's data. But, a greater offence consists in representing that extract, mutilated or un-mutilated, as announcing the foundation of the whole of my author's work, and in then proceeding to confirm that imputation by a false translation of a long paragraph which had no relation to my specific subject.

The reader will not be surprised to learn that I have not yet presented the whole of the fraudulent extract. The foregoing is the last part of it, and the following is the first part.

Rejected Translation.

"In order to be able to make up my opinion upon a question concerning which one could decide very little by means of simple discussion, *I collected* between the years 1822 and 1827, the histories of all the patients suffering from acute diseases, which were admitted into the hospital of La Charité, in the wards St. John and St. Joseph, at that time under the superintendence of M. Chomel."

"Revised and altered" Translation.

"In order to make up my mind upon a question which simple discussion would not tend to elucidate, *I examined and recorded*, between the years of 1822 and 1827, the histories of *all the patients* affected with acute disease, that were admitted to the hospital of La Charité *in the apartments under the supervision of Mons. Chomel.*"

The suppressed Original.

"Afin de savoir à quoi m'en tenir sur une question que ne pouvaient pas beaucoup éclairer de simples discussions, *j'ai recueilli*, de 1822 à 1827, l'histoire de tous les sujets atteints de maladies aiguës admis à l'hôpital de la Charité, dans les salles Saint-Jean et Saint-Joseph, alors confiées à M. Chomel."

The reader will perceive that the old translation is the true one, whilst the new is false ; the words "*I examined and recorded*" being

substituted for "*I collected.*" This was designed, also, to make it appear that the 900 cases should be considered as having been "examined and recorded" by M. Louis, just as the 138 cases of typhoid fever had been, and that the whole, therefore, should be allowed to form equally the basis of his work. To carry this effect more fully, my critic has placed the words "*all the patients*" in Italics in the new translation,—the same being in the Roman letter both in the old translation and in the original work. The other Italics are mine. By this process, also, he strengthens the charge as to my having falsely represented my author as to the foundation of the anatomical doctrine, specifically, and in a general sense as to the common basis of the work.

It is also worthy of remark, that the translation of the words *j'ai recueilli* "I examined and recorded in one instance, and "I carefully recorded" in the other, and the connections in which they stand, show forcibly the *quo animo* of the writer; whilst, if any charity could be extended to the translation of *j'ai recueilli* "*I carefully recorded,*" it is dissipated by the translation of the same words "*I examined and recorded,*" and the farther fabrication of the words "*when examining.*" But, the only meaning which *j'ai recueilli* can possibly bear is that as artlessly rendered by Dr. B. in his original version; and that such is truly his opinion is shown by its translation, in one instance, "I collected," and in the other, "I learned,"—both occurring in one paragraph, and relating to the same subject.

This affair being destined to occupy a conspicuous place amongst literary curiosities, I will indicate one aspect more in which my critic has *fixed* the matter for my discomfiture and for the benefit of "copyright." This will appear by taking together the whole paragraph which immediately follows the quotation which I have been employed in *unfixing*. The reader will then have the whole before him, from the beginning of the mutilated extract from my work, to the end of the subject in its direct aspect. The following words which I have placed in Italics are intended by Dr. B. to confirm M. Louis's extreme and equal accuracy of observation in respect to the 900 cases, and to carry more conclusively the imputed offence of suppression or misrepresentation. Thus:—"One would think that these facts were sufficient to enable one to come to some definite (we will not use 'conclusive,' as it offends our commentator so much) results. 'In my analysis,' *continues Louis, 'I have wholly left out any facts which were not sufficiently exact—and when I have deduced any consequences, I have always kept before me this idea by the author of Emile, I know that truth resides in things, &c.'* In a note to this paragraph Louis informs us that he threw aside as incomplete all the 'observations' made during his first eight months of *devotion* to these studies. One would think that the *accurate examination* of about 1000 cases ['nearly 900'], and the autopsies of 1–10 of them, would have enabled any accurate observer to decide whether a lesion was *unimportant* or not.

"*So much for Louis's data and accuracy of observation of nature.*"

Here the strong intent of Dr. B. to misrepresent me, and to make it appear that M. Louis bestowed as much personal care upon the whole

900 cases, and intended the whole to form equally the basis of his work, as the 133 cases of the typhoid affection, is again forcibly shown. And, to make the deception more effective, my critic introduces into the last quotation an extract from M. Louis, in which he (M. Louis) says "in *my analysis* I have *wholly left out* any facts which were not sufficiently exact,"—Dr. B. meaning to imply by this citation, in its connection with the whole paragraph and the preceding, that M. Louis *equally* employed in his "analysis" the entire 900 cases. And, to make this still more forcible, he refers to a note by M. Louis in which he (M. Louis) threw aside certain other cases. Now in this note, M. Louis adverts specifically to the 900 cases, and (without invalidating those), he does not limit the rejected ones, as Dr. B. implies, to "his first eight months," but leaves it indefinite. Thus :—"Among *those* which I did not consider sufficiently accurate, were all those which I collected during the first eight months of the six years," &c.

But the foregoing intent is rendered more conspicuous by the artificial and emphatic translation of the words—"In my analysis I have *wholly left out* any facts which *were not* sufficiently exact."

Now, what was my critic's first translation?

"In these analyses I have not made use of any records of disease which *did not appear* to me to be sufficiently exact."

And, what the French?—"J'ai retranché des matériaux de mes analyses les faits qui m'ont paru manquer d'un certain degré d'exactitude."

There are some minor circumstances attending the management of the foregoing new translation which go to show its extensive bearing, and the ingenuity with which it is executed. The reader will doubtless observe them; but I will detain him by indicating one which has a bearing upon a subject already examined. It relates to the question (so unimportant in principle) whether M. Louis had the actual treatment of the typhoid patients. It should be premised that I have observed the punctuation as it occurs in all the extracts which relate to the subject under investigation; and here, by the way, it will be seen that the new punctuation itself is sometimes designed to coöperate with the altered version.

Now, where it is announced by M. Louis, that Chomel had the supervision of the hospital, Dr. B. translates the French in the manner already stated, but which I shall here repeat for the convenience of the reader.

Old Translation.

"I collected between the years 1822 and 1827, the histories of all the patients suffering from acute diseases, which were admitted into the hospital of La Charité, in the wards St. John and St. Joseph, at that time under the superintendence of M. Chomel."

New Translation.

"I examined and recorded, between the years of 1822 and 1827, the histories of *all the patients* affected with acute disease, that were admitted to the hospital of La Charité *in the apartments under the supervision* of Mons. Chomel."

All the Italics are mine, except "*all the patients*," which my critic introduced for his own use, as already shown. The reader will see that the words "*St. John and St. Joseph*" are expunged from the new

translation, as these are of specific import, and show that those two wards were probably appropriated, more especially than others, to M. Louis. In the original, and in the discarded translation, "the superintendence" evidently refers to the hospital at large, whilst a distinction is implied as to two of the wards. In the "revised and altered" translation, however, that distinction is obliterated.

"Thus, tho' from truth I haply err,
And sacrifice my character,
What man of taste my right will doubt,
To put things in, or leave them out?
'Tis more than right, it is a duty
If we consider landscape beauty.
He ne'er will as an artist shine,
Who copies nature line by line;
Whoe'er from nature takes a view
Must copy and improve it too:
Thus I (which few, I think, can boast)
Have made a landscape of a Post."—"Dr. Panglos."

Having already trespassed too far upon this week's Journal, I shall defer my remaining examination of this involved subject. At the close of my fourth number, will be also found some remarks having an important relation to it.

No. 3.

THE most important objects of Dr. B., as investigated in my last number (imputing misrepresentation to me, and placing the 900 cases on an equality), pervade his attack. Both are fundamental points, and are urged to show a general defect in my conclusions from their neglect of a primary element of my author's work. The reader is also thus diverted from the main object of my Essay, which is to show the pathological and therapeutical doctrines of my author, the amount and nature of the anatomical investigations, and how far these investigations were made the foundation of important pathological inductions. With these last I was especially concerned, and it was my object to prove that these were mainly founded upon the anatomical results of 133 dissections, of which 50 were cases of the typhoid affection. There were, however, certain cutaneous phenomena, meteorism, and all that related to fixing the typhoid disease at its incipient stage upon Peyer's glands, which involved the 88 cases of the disease that terminated in health. And, as those phenomena gave rise, respectively, to important generalizations, it was all carefully stated in my Essay. To appropriate these last generalizations to the typhoid affection, it was necessary to compare the cases with the vital phenomena of other diseases; and the residue of the 900 cases forms the ground of that comparison. This is the *full extent* of

the importance they can possess under any circumstances ; nor have the therapeutical conclusions the most remote relation to them.

But, Dr. B.'s perversion even exhibits me as having perpetrated the absurdity of supposing that M. Louis, whom I represent as having passed much of his life in hospitals and the dead-house, did not possess the means of comparing his 138 cases of typhoid fever with other diseases, and that I had not specifically admitted the fact. Nevertheless, I objected to the assumption of a greater number than the 138 and 83 cases, partly upon the ground of M. Louis's violent rejection of cases by others which are not accompanied by more substantial proof than his own, but mostly because I have shown that his pathological inductions, with the foregoing exceptions, are founded upon "the debris of the body." I have expressed no doubt of the vast extent of M. Louis's observation, especially at that time, in hospital practice. It would have been absurd to have questioned a fact so notorious. And though the great experience of my author required no admission to that effect from me, I regarded the results of that experience as exhibited in his works. I attempted to show, therefore, that he was not an observer of nature, but of the ruins of nature, and that his pathological doctrines are founded mainly upon the latter. I did not think this the *sort of nature* which, alone, makes a good book of "laws" and generalizations in pathology. I fully admitted my author's detail of vital phenomena, but I also endeavored to show that they were not rendered practical elements in his pathological data. A primary object of this, was to supply a practical demonstration of the validity of what I have said in my Essay on the "Comparative Merits of the Hippocratic and Anatomical Schools" as to the errors of the *exclusively* anatomical. I was concerned about his *book*, its doctrines and their ostensible ground ; and my objection to any of his cases was not predicated of any real distrust that he had put forth what was not in itself substantial, but of his own requisitions and habits in relation to others. This I stated, that it might pass for what it was worth.

It being, then, my duty to give a fair representation of my author's work as it is, I laid out my ground, plainly, fairly, and fully ; and, in so doing, I allowed the whole extent of any experience which it might be supposed that M. Louis had employed ; though fidelity to my task compelled me to place the true basis of my author's generalizations in pathology and therapeutics in its proper light. At the opening of my Essay, I state that, "our author has occupied the proud elevation of presiding over the modern anatomical school ; and it becomes, therefore, a matter of deep interest to inquire, through his remarkable labors, into the *practical* results of morbid anatomy, and how far it may have advanced, or retarded, the progress of medicine. The present Essay, therefore, may be considered *so far* a continuation of the last,"—or the Essay on the Hippocratic and Anatomical Schools. It became me to show that his generalizations in pathology were founded upon the debris of 133 subjects, the greatest amount that is stated by my author. This I have done, and most extensively, in his own words. And, although an analysis of only 54 cadavers is given, I allowed the whole that was claimed,

and even all the subjects (88) that recovered of typhus fever, but whose cases, also, are not analyzed according to M. Louis's principles. Doubtless they had been; "but he should have given us the proof." This is his doctrine with past observers. To all the foregoing, however, I made no objection, as they entered, unlike the residue, into my author's most important conclusions,—especially the 83 dissections in divers maladies. But, did I not do more than this,—aye, even more than allow the 900 cases, with the qualifications I have mentioned? Did I not finally release M. Louis from the restraint to which he was subjected by a fundamental rule of the "numerical method," and to the observance of which he had laid himself under a deep obligation?

But, I must speak by the book; for doubtless my critic would otherwise deny that there is a word upon the subject beyond what he has presented in the mutilated extract; whilst, also, in so doing I shall protect his veracity. I will take for an example a statement which appears at the very opening of the Essay. After saying "it would be *unimportant* whether our author was the *acting* attendant, since he makes the cases entirely his own, erects generalizations upon them, and defends the whole treatment," I go on immediately thus:—

"And here it is important to bear in mind, that all our author's conclusions as to this affection were founded upon '138 observations of typhus fever, 50 of which were relative to individuals who died of it;' and, that he 'compared' this exact number of 50 cases 'with the alterations found as consequences of other acute diseases, in 83 subjects whose histories I learned.' Upon these cases, and the *lesions observed in other diseases*, our author founds those conclusions which will appear in the sequel."—(P. 635.)

Here, then, by the words "*lesions observed in other diseases*," as well as by the statement of 138 cases, Dr. B. is farther rebuked, and truth avenged. And then I went on thus:—"In the mean time, let us observe, also, in relation to the Typhoid Affection, that it is stated by our author, that—

"'In these analyses I have not made use of any records of disease which did not appear to me to be sufficiently exact, and whenever I have made deductions from those which were exact, I always kept in mind the thought of the author of *Emile*. 'I know that truth lies in the facts, and not in the mind that judges of them, and that the less I introduce what is merely my own into the deductions I make from them, the more certain I shall be of approaching the truth.'"

Thus, my reader had before him my author's premises and intentions; and, what I believe is not often practised, I sought for them in the "Advertisement," where alone they are announced. I have also given him, everywhere, the advantage of stating, in his own language, his various objects, all his "rigorous conclusions," "laws," &c., and all their contradictions; and it is this fairness, beyond doubt, which has most annoyed my critic. Next to that is the proof which goes to show the foundation of my author's pathological generalizations upon morbid anatomy. This was the fundamental purpose of my Essay, and without it, it could have had no existence. And next to that, Dr. B. is discon-

tented with my demonstration, that all my author's particularities about symptoms, and all his cases beyond those of mere morbid anatomy, do not enter into his important generalizations in pathology. They are a mere show of words, and it was a special object with me to prove them such. My readers will find the proof by turning to the Essay itself.

But, let us have an example of my manner of objecting to a greater proportion of the 900 cases. After stating for the second time, the premises which relate specifically to the typhoid affection, I went on thus :—"Although the inductions are avowedly founded upon the foregoing number of cases, we are disposed to allow *any latitude of observation* which it may be supposed our author can have made. Nevertheless, it would be more correct to apply to our author the rule by which he restricts all others, and of which the following is one of the numerous instances of its derogatory insinuations :—

" 'Corvisart does not state *how he proves* the truth of the assertion which he *tries to explain*. The reader may seek for the proof in his work, but where will the proof be found? It may be answered to this, that Corvisart had not observed those cases merely, *the histories of which he has published*, but that *his assertions* rest upon a *much larger number*. But, even in this case, Corvisart should have counted, and if he had done so, he would have stated the fact, since the question was simply one of number.' "—(P. 693.)

Here, too, it is evident, that the statement of the number alone would have only protected Corvisart against an impeachment of his veracity ; since my author would have rejected, as is implied by the extract, and according to his well-known rule, so little observed in his treatise on Bloodletting as well as on the Typhoid Fever, all the cases that did not bear the "*proof*" of a critical analysis. In this respect he is imitated by our translator, who says he is "unwilling to take the assertion of any man"—(P. 75.) This remark terminates the paragraph in which it is said that "Dr. Paine kindly consents to take Dr. Hale's assertion for the truth of the results of 197 cases,"—my true remark being intended to discourage this want of professional confidence. I was speaking, in a note, of Dr. Hale's objections to Perry's 4000 cases of typhus fever, in which he made 300 anatomical inspections. Now what does Dr. Hale do in this instance? Certainly, what I have not done in relation to M. Louis, as has been just seen by an extract from my Essay. Perry does all that Dr. Bowditch would require of M. Louis. That is, he states the number of his cases, 4000, and the dissections, 300, and gives us the result. These, too, were cases of typhus fever, whilst those which M. Louis announces in a round number were made up of various diseases.

Nor can I permit my objector to represent me as trifling with Dr. Hale. I must, therefore, bring the misrepresented statement before the reader. These are my words :—"It is objected (by Dr. Hale) that the '*particulars* of Perry's 4000 cases, and 300 inspections, are not given so as to enable us to judge how far he might be influenced in his observations by theoretical views, or with what degree of care and thoroughness his observations were made.' We would not have ob-

jected to this from M. Louis, but, whilst Dr. Hale gives us an analysis of only four of his 197 cases, we confide most implicitly in his general statement as to the remaining 193."—(P. 690.) Now Dr. Hale is justly esteemed an ornament of the medical profession. He inclines to the numerical school, so far as figures and analytical proof are concerned. He rejects Perry's 4000 cases and 300 dissections, because "the *particulars* are not given," &c. We might well, therefore, take his authority as to whether a greater part of M. Louis's 900 cases are entitled, upon the principles of the school, to any greater consideration,—and this more especially as M. Louis had in contemplation a multitude of "*theoretical views*."

And, what says Dr. Bowditch, who is "unwilling to take the assertion of any man"? "The numeralists," he says, "*must have an analysis of facts recorded as they actually occurred, and at the time of their occurrence, or they will not be satisfied.*"—(P. 76. *My Italics.*) This lets us into one of the motives for the new translation of the long paragraph, which was examined in my second number; and, doubtless, my reader has anticipated me in another, which I shall, however, now indicate. The surreptitious translation is a clear proof that Dr. Bowditch does not consider any of the cases beyond the 138 of the typhoid affection entitled to the consideration which he professes should be ascribed to them; otherwise he would have never fabricated so many words that are expressly intended to give the residue an equal importance, according to the new method. The fabrications, therefore, which are introduced into the new translation of the fundamental statement, completely divest all but a few of the 900 cases of any authority; according, also, to my translator's object and avowed doctrine. And that Dr. B. truly believes with me that the pathological and therapeutical inductions of M. Louis are founded upon the 138 cases of typhoid fever, appears from a declaration which he subsequently makes, and by which he restricts the whole anatomical premises to the 50 fatal cases of that disease. Thus:—"But, at present, we must conclude, both from Chomel's and Louis's researches (82 fatal cases collected during a space of 12 years by two eminent men), that the anatomical characteristics of typhus in Paris are as have been described."—(P. 90.) *Fifty* of these 82 cases go to form the substantial part of M. Louis's pathological inductions as to the typhoid affection.

I am now prepared to affirm, what I did not in my Essay, that the inductions of M. Louis, pathological and therapeutical, in his work on the Typhoid Affection, are virtually founded on 138 cases of that disease, and upon no other, except in a negative sense which relates to an experience of which we have not a "proof" in conformity with the doctrine of the author.

Having now unravelled a snarl which it was evidently supposed must remain untouched, from the extent of explanation which would be necessary; having made out fully all my specifications, and the ends of justice being substantially answered, truth avenged, and my critic disarmed, it may be thought that generosity should arrest my pen. It falters, indeed, as the allusion escapes; and here should it pause, but

that the fabrication, whose analysis I began in my second number, is made the substratum of most of the misrepresentations which compose the details of my critic's article. There is nowhere any attempt to controvert, with the usual honesty of reviewers, my facts and my arguments; but it is one unbroken series of defamatory charges, and ribald derision of my work. My adversary, therefore, lacking so fundamentally, and being destitute of the redeeming, though pretended, virtue of shielding another's fame, it has appeared to me that the whole force of his assault should recoil upon himself.

No. 4.

DR. BOWDITCH states, that,—

"Dr. Paine never suffers his reader to lose sight of the *main object of his two volumes*, viz., a violent attack upon the numerical, or, as he chooses to call it, the anatomical school."—(P. 73.) This is repeated, thus,—“the numerical (or anatomical, according to Dr. P.) school.”—(P. 74.) And yet again:—“It would seem, then, that our commentator raised up nothing but a spirit; and we find him fighting *as a fundamental point* of the numerical method, a chimera of his own brain. This *two-fold error* of supposing the numerical and ‘anatomical’ schools *identical*, and that the *former trusts* to pathological anatomy as the ground-work of its system, *runs through* the whole of the hundred pages of criticisms.”—(P. 79. *The Italics are mine.*)

I know not how to characterize these atrocious misrepresentations. I would most willingly avoid all offensive epithets; and it is only the distinctness with which I shall show their desert that I can offer as a justification.

My work, in a general sense, aims at very different, and far higher objects. It is extensively concerned about nearly all the great subjects in physiology, pathology, and therapeutics; and a *practical bearing* is everywhere given to the whole. Its general scope is to inquire into the prevailing philosophy in respect to each,—to indicate the methods of inquiry which should be observed by philosophers,—to exhibit the evils of hypothesis and the advantages of true theory,—to analyze our existing knowledge in the various branches examined,—and to wrest our facts from the grasp of speculation and reduce them to fundamental principles. Such are the objects of my work, and whether accomplished or not, it equally stamps the foregoing representations of my critic.

Let us now look at the statements more specifically. Dr. Bowditch presents me before the world, and before my work is known to the public, as having been guilty of the absurdity and injustice of “calling the numerical, the anatomical school,” and of making them “*identical*.”

To enforce the intended effect, he also affirms that it is the "main object of the two volumes" to carry on "a violent attack upon the numerical, or, as he chooses to call it, the anatomical school."—(P. 73.) Now, in the first place, I have devoted an essay, of 37 pages, to a consideration of the "Comparative Merits of the Hippocratic and Anatomical Schools." Here, then, if anywhere, this imputed confusion should appear. There is not, however, in this Essay, a remote allusion to the numerical school. That school forms a special, but brief subject of consideration in the Essay which is devoted to the "Writings of M. Louis," and in my "Philosophy of Bloodletting." Morbid anatomy I have everywhere advocated, and have labored to point out its legitimate and important relations to pathology; but always holding it in subordination to the vital signs. The numerical school, in *M. Louis's* *acceptation*, I have condemned with as little ceremony as he has all preceding observation; nor have I any apprehensions of my success in this respect. It will be thus seen that Dr. B. has a comprehensive purpose in the foregoing statement. It represents me as deficient in common understanding,—my work as hostile to morbid anatomy as to "numeralism," and without that element in its foundation. A distinction is even cautiously maintained between the anatomical and numerical schools, which have no other than an incidental connection. At the beginning of my Essay on the Writings of M. Louis (p. 683), after saying that its principal object is to exhibit "the practical results of morbid anatomy," and that it "may therefore be considered so far a continuation of the last" (or the Essay on the Hippocratic and Anatomical Schools), I add, that, "we have also bestowed *some notice* in our first volume upon our author's *numerical system* in its relation to bloodletting; but, we shall *transiently notice it again in other aspects*."

Again, however, there is no incompatibility between the two schools, whatever may be the abstract distinctions. The "numerical" may take the lead in the anatomical, and embarrass it with absurdities. But this only proves their elementary distinction. Bichat and Hunter were general anatomists of the highest consideration, each variously dissecting, but always rendering necroscopic results subordinate to vital phenomena. Therefore is it, that comparatively little of the former appear in their writings, especially those of Hunter. Their pages glow with nature as she glows, and they have but little of the odor in which the strict anatomist delights. The dross of the dead-house is subjected to an alembic, and a sweet distillation imparts its delightful perfume. Hence I have said in my work, that,—“Coming to Hunter, we find him analyzing the principle of life, and expounding the whole philosophy of inflammation, with scarce a reference to a post-mortem examination” (*Vol. 2*, p. 674); whilst, in another place I have it,—“who was neither *anatomist*, physiologist, surgeon, nor naturalist alone, but the most remarkable combination of *all these* that the world has yet seen.”—(*Ibid.*, p. 538.) All this is exactly true. Of Bichat, I have said,—“It is not, then, the *great architect* of the *anatomico-pathological* school, of which we are a *humble advocate*, who laid the foundation wrong; but the glare of his light was too much for his countrymen, who, as Armstrong expresses it,

'have just burst from the old system of pathology,' and who, as avowed by Travers, are unacquainted with the achievements of Hunter."—(*Ibid.*, p. 663.) My opposition related to the "excesses of the dead-house" (p. 663), not to morbid anatomy as cultivated by Hunter and Bichat. But, morbid anatomists as they were, think you that either would have tolerated "*the numerical school*"? Andral and Louis are placed, by common consent, at the head of the present anatomical school, whilst Broussais is ranked as a chief by a reviewer with whom I had the honor of an argument.—(Vol. 2, p. 645.) But, would Andral esteem it a compliment to be classed with the "numeralists," and did not my critic fear that the ghost of Broussais might call upon him for atonement?

Again, Dr. B. is pleased to say that "there is scarcely a hundred successive pages in either volume, in which this opposition does not manifest itself,"—that is to say, "a violent attack upon the numerical school,"—"the main object of the two volumes." Now there is no allusion to the subject, unless very incidentally (and of such I have no recollection), in any one of my Essays, excepting in those upon Blood-letting and the Writings of M. Louis. In the former, a short space only is devoted to its consideration, and I only recurred to it briefly in the latter. Here, in this Essay on the Writings of M. Louis, Dr. B. gives to his reader the *impression* that I have "dedicated a whole chapter of 134 pages" to exploding the "bigoted numeralists."* Now it happens that I have "dedicated" only three pages of this Essay to that object, and these are *isolated* from the rest by a black line drawn at their commencement and termination, that the individuality of this part of my subject might be at once obvious to the most careless observer. These three pages begin with the following sentence. Thus:—"Of the *numerical* method we have, perhaps, said as much as is incumbent upon us in our first volume, pages 293—309, 332. We have there exhibited an instance of its practical application in the hands of others (p. 305), and we will now present an illustration by our author."—(P. 780.) Indeed, Dr. B. has unwittingly indicated the amount of attention which I have given to the numerical school. Thus:—"Our author progresses in his zeal, and devotes *three* pages (Vol. 1, p. 293) to the improprieties of the numerical school" (p. 76), and just this amount, as I have shown, is given to the subject in my article upon the Writings of M. Louis. This Essay is mainly taken up in indicating the abuses of morbid anatomy,—in showing the generalizations of my author which he founded upon them,—in pointing out the dangers of false philosophy,—in arraigning my author for a departure from his own rules in philosophizing,—in protecting my brethren, from Hippocrates to the present time, against his almost universal scorn and derision,—in demonstrating the fallacy of the assumption that "medicine is now in its infancy."

* Pp. 73, 74. Dr. B. quotes these two last words with the manifest intention of imputing the epithet to me. Not having had the leisure to look over my work, I will not positively affirm that such an expression may not have escaped me. I believe, however, that the word "bigoted" is not in the book. The use of the word "strangled," in connection with bloodletting, is also imputed to me. (P. 75.) It is possible I may have committed the barbarism; but if so, I call upon Dr. B. to publish the remark, as well, also, that in which the words "bigoted numeralists" are said (by marks of quotation) to have occurred—the volume, and pages. If I am convicted, I fully allow the justice of the criticism. If I am not, * * *

The foregoing misrepresentation is also extended to the anatomical school. Now this school does not come under consideration in my first volume, excepting where I speak, in my Essay on Bloodletting, of the numerical method, and where, also, I bring forward morbid anatomy to *sustain* my argument with Dr. Hall as to the imputed effects of excessive bloodletting. And here, by the way, the few pages which I have devoted to the *numerical* school are also isolated from the section by a line. The subject, too, as in the instances already cited, begins with a statement which makes a radical distinction between the numerical and anatomical schools. Thus:—"We shall now proceed to an examination of the treatment of simple pneumonia, and of some other inflammatory affections as founded upon the 'numerical method' in *its connection* with morbid anatomy *as cultivated* by the *exclusive* philosophers of the *anatomical* school."—(Vol. 1, p. 293.) It will be recollected, too, that Dr. B. refers to this very page, and had he fairly presented the subject in the foregoing acceptation, and called it *Louism*, I had not objected.

Coming to the second volume, I have nothing on the subject of the anatomical school in my Essays on Animal Heat, the Philosophy of Digestion, and the Theories of Inflammation; unless in the last indirectly, and not in the sense imputed by my reviewer. In my Philosophy of Venous Congestion, there is nothing upon the anatomical school, unless a casual reference, and here I bring up morbid anatomy, *and far more extensively than has been allowed*, as an *auxiliary* to my pathology of that disease. Finally, it is not, till near the end of the second volume, where my article on the Schools is placed, that the question is brought forward. In my Philosophy of Venous Congestion, a remark occurs which farther places this subject and my critic in their proper attitude. Thus:—"In our Essay upon the Comparative Merits of the Hippocratic and Anatomical Schools, and in our examination of the Writings of M. Louis, we have endeavored to show the superiority of the vital signs in marking the true pathology of disease."—(P. 316.)

Now, as to the second part of the "two-fold error," "that the *numerical* school trusts to pathological anatomy as the ground-work of its system." If M. Louis be assumed as *constituting* "the numerical school," then is the affirmation correct; otherwise it has no foundation. No one venerates more than myself the philosophical habits and labors of many (as Jackson, Hale, Gerhard, &c.) who employ the numerical method as an *auxiliary* to medical science. This remark leads me to say that I have been misapprehended in my views of the method, and perhaps I may not have been sufficiently explicit. Now it so happens that *I like the method* without its abuses, and as such have long employed it. Whenever I have spoken of it, I have always intended M. Louis's "numerical method," which rejects all observation that is not founded upon it, brings pathology and therapeutics under the dominion of mathematics, regards not the various considerations which relate to climate, constitution, habits, age, sex, &c., and practically knows little else than a *balance sheet*. As the method existed prior to this innovation, it offers important advantages as a general memorandum. It is of

M. Louis's "method" and "numerical school," therefore, that I have spoken in my work. I would advocate the old method, or, as Dr. B. has it, "the plan originally proposed by others," and "we hereby give in our faith."—(P. 78.)

Dr. Bowditch brings me forward as the champion of Chomel. "Dr. Paine," he says, "seems to think himself called upon to defend the reputation of Chomel," &c. Then follows a long exposition, implying that I have conveyed an impression that Chomel undervalues his friend Louis. The latter implication is without a shadow of foundation; and, as to the former, its only source is my remark that, "this distinguished observer, however, should be in no respect associated with our author's performance" (p. 685), and this, as will soon be seen, was modified immediately afterwards. Although I have now stated the whole of the "defence," Dr. B. nevertheless has it—"as our author thought it necessary to devote *two pages* to the subject, we were unwilling to pass it by unnoticed."—(P. 79. *My Italics.*) It is certainly painful to me to call up subjects of this nature, unless for the purpose of some general interests. Yet, being required to meet a calumniator, I must show the reader with what reference to truth I have been presented as the "defender" of Chomel. This will appear farther from the following extract from my work. Thus:—"And, *coming to the writings of M. Louis*, we shall make it a particular object to inquire how far both himself and *Chomel* are entitled to the rank which is awarded to them by the able writer who stands at the head of this Essay, as we have already endeavored to show with what consideration 'M. Andral is allowed by the profession to be the first pathologist in the world.'"—(Vol. 2, p. 645.) And now as to the charge of having placed Chomel in opposition to his friend Louis (p. 79), take the following passage, from my Essay:—"Not only such an example, but a multitude of them, occur in the writings of an author who *will not be suspected of any disposition to interfere with M. Louis's generalizations*. This author is Chomel, so *largely interested in the works on Typhus, and Phthisis, now under consideration*."—(P. 687.)

This last quotation carries us back to my 1st No., as it bears directly upon Dr. B.'s statement of my denial of Chomel's "superintendence" of the hospital. The remark occurs, also, within two pages of my general statement of the fact, as expressed in the "Advertisement," and of the misrepresentations which I am now investigating.

In reference to the controverted anatomical lesion of Peyer's glands, my commentator affirms, that,—“Among the ablest and worthiest, and the one upon whom Dr. Paine rests his greatest hopes, is Chomel;” and, to carry out this deception (and the memorable one connected with it, as shown in my second number), Dr. B. troubles the subject at great length. (P. 81.) Now, although I have introduced Chomel as the first of my witnesses because he was the *earliest* in order as to time, it will be seen by referring to my Essay that he is one of the *least* in importance; nor have I brought his testimony as having been arrayed (according to Dr. B.) against M. Louis's conclusions. On the contrary, it is immediately preceded by the last quotation from my work (p. 687).

I have, also, even invalidated his statements with the "numerical school," by saying that his "facts are rather *old*, but little known, and more frequently neglected." I cannot, therefore, consent that my greater witnesses, Tweedie, Lombard, Perry, Craigie, Alison, The British and Foreign Medical Review, Dunglison, Geddings, Hale, &c., should be crowded aside for the benefit of the master. It is owing to this large body of proof, clear and decisive, and the inferiority of Chomel's, that Dr. B. misrepresents the latter, that it may appear to follow that I have not aided in putting at rest a question upon which much of M. Louis's fame has rested. By turning to pages 688—692, the reader, who may have the curiosity, will see how the case stands between Dr. Bowditch and myself.

The perverted extracts which formed the subject of my second number are preceded by more than a page of introductory comment, which relates exclusively to the controverted anatomical lesion of Peyer's glands. The reader is here prepared for the coming event by imputing to me an unfair representation of Chomel's opinion of that lesion. This charge is conveyed in a manner so well introductory to a succession of deliberate acts of the very nature of the offence which they fabricate and condemn, that the reader will be gratified with seeing it. Here it is:—"The reader doubtless will suppose, from what we have extracted from Dr. P.'s remarks, that Chomel believes that the peculiar lesion of the intestinal follicles ascribed by Louis to the typhoid affection can be found in many diseases. Now we deny that Chomel ever said so, or meant to be understood so to say; and we assert that he declares exactly the contrary, and that it is Dr. Paine's *garbled quotation* that has led the reader into error."—(P. 82. *My Italics.*) I cannot encumber my reply with extended quotations from Chomel to show my translator's misrepresentation. But, as it was designed mainly as a *ruse* to give effect to the greater fraud which followed, I shall now state all that occurs, in my work, upon this subject in relation to Chomel, and of which Dr. B. predicates his affirmation that,—"*Among the ablest and worthiest, and the one upon whom Dr. Paine rests his greatest hopes, is Chomel.*"—Thus:—

"Chomel states that alterations of the glands of Peyer were common in the epidemic Parisian cholera of 1832; so common, indeed, that he was disposed to carry M. Louis's philosophy of the dependence of typhus upon the glandular affection to the cholera itself, and to establish an *affinity*, if not the identity, of these diseases. [*This is true.*] It is certain, that such had become the ascendancy of the glands of Peyer in 'pathological anatomy,' that, as in the typhoid affections, those glands were regarded by the savans of Paris not only as the veritable *seat* of cholera, but as the cause of its morbid phenomena. [*And this is true.*] Thus:—'*On se rappelle que, dans l'épidémie de 1832, les premiers observateurs qui eurent l'occasion d'ouvrir des corps de cholériques crurent avoir trouvé dans la lésion des follicules intestinaux, et le siège de la maladie, et la cause de la plupart des phénomènes morbides qui la caractérisent.*' But, this is not all which Chomel supplies. He has seen the same alteration of the glands of Peyer as attends typhus, in

scarlatina, and other affections (sujets morts d'affections différentes) ; with the *exception of the ulceration*. [*And this is true.*] He states one case, in erysipelas of the face."*—(P. 688.) *This is also true*, and the end of my "ablest witness"! The reader will also not fail to observe, in the first part of the extract, how far I had placed Chomel in hostility to Louis.

The reader, by referring to Chomel's work, will see that there is no "garbled quotation," no words *fabricated*, or *expunged* from the quotation, and that my author's statement is fairly expressed. Nor should I neglect saying, that it was my principal object, in quoting Chomel, to show, through him, what had been the opinion of the "scavans of Paris" as to the dependence of *Cholera* upon the follicular lesion. This is my great proof from Chomel, and it is undoubtedly important.

It will be seen, also, that I have stated the only exception of the least importance (that of "*the ulceration*"), as it respects Chomel's own cases, that the reader might judge for himself, whether the follicular lesion observed by Chomel in "other affections" than the typhoid, was not so nearly allied to that which had been observed by M. Louis in the latter disease, as to divest the typhoid affection of its imputed characteristic. This was my own opinion ; but I did not say, as alleged by Dr. B., that it was the opinion of Chomel. I was merely stating a fact ; and if any one thing, more than another, can show the trifling with morbid anatomy, and that it is made the basis of pathology by Louis, Chomel, and Dr. Bowditch, it is the very pottering about this lesion of Peyer's glands, and whether, as to Louis, at least, it shall be the *seat* and *source* of all the structural lesions and all the vital phenomena in the typhoid affection, when it may be *ulcerated*, or appertain to some other disease when it is *not ulcerated*, but having essentially all the other morbid attributes.—See *B.'s attack*, p. 82.

But, I have a few words more as to this charge of falsely representing the facts from Chomel. This is predicated of a fraudulent extract from my Essay. I shall place the right and the wrong ones in parallel columns.

My Remark.

"Chomel has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina and other affections (sujets morts d'affections différentes), *with the exception of THE ulceration.*"

Dr. Bowditch's Quotation.

"Dr. Paine says that Chomel 'has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina and other affections (sujets morts d'affections différentes).'"

The reader will see that the important qualification is *expunged* ; and having committed this act, Dr. B. proceeds *immediately* to say, "We are sorry to see such a lamentable deficiency in the fairness which we expect in one who *quotes*!"—(P. 82.) This was a fitting preliminary to the other mutilated quotation, and the false translation, which follow in immediate connection, and which were the subject of remark in my second number.

A fundamental anatomical question will begin my next number.

No. 5.

DR. BOWDITCH "denies entirely the truth of the assertion, that Louis or the numerical school do make pathological anatomy a *paramount* guide in pathological inquiries; and Dr. P. must know little of the matter when he accuses Louis of thus using it."—(P. 78.)

Here the reader will see an involution which has been more directly presented in other places, viz., that of making me extend my premises which relate alone to M. Louis, to the whole "numerical school," and thus, also, to maintain the imputation of confounding the anatomical with the numerical school. The statement, however, falls, of course, under the denomination of all the others. But, let me in the first place say, that it may be far more correctly affirmed of the anatomical than of the numerical school, that the former does make "pathological anatomy a *paramount* guide in pathological inquiries." This is notorious the world over; and it is so emphatically true of M. Louis, that nothing evinces the assurance and folly of Dr. B. more clearly than its denial. It was the great object of my Essay on the Hippocratic and Anatomical Schools to show the advantages of the former in making anatomy subservient to the vital phenomena of disease, and to indicate the evils which have resulted from the opposite system, and from which the latter school derives its very name. In respect to M. Louis, I had mainly in view, as I have already said, a *practical illustration* of the sad results of "making pathological anatomy a *paramount* guide in pathological inquiries;" and as to his "numerical method," I was scarcely concerned (and only in three pages) about that stupid affair. "It is as mechanical," says M. Double, "as the employment of a shoemaker."

But, let us have something directly to the point,—something from the *master* himself, be he anatomist or numeralist. In the work on Phthisis, Louis thus lays down the doctrine:—

"We have divided our work into two parts, and since ANATOMY IS THE STRONGEST SUPPORT OF PATHOLOGY, we have commenced the analysis of our facts by a general description of the *visceral lesions*."—(*Preface*, p. 70. *My capitals and Italics*.)

I have also copied the foregoing declaration from Dr. Bowditch's "revised and altered" edition. Again, practically:—

"We must infer that it is in this last lesion (the glands of Peyer), and *not in any other*, that we must look for the cause of the *delirium*, and more especially of the *somnolency*," in typhoid fever.—(*On Typhoid Fever*, Vol. 2, p. 132.)

The foregoing anatomical doctrine runs through the works on Phthisis and on the Typhoid Fever. In the Essay devoted to the writings of M. Louis are hundreds of extracts setting forth his generalizations which were founded upon the "debris of the body." No small part of Europe has been long at war with him upon this subject. The contest began very early, and that distinguished philosopher, Dr. Jackson, wrote several years ago, after the following manner:—"It is objected by some to

the labors of M. Louis, and others of the French pathologists, that they labor, indeed, with ardor on the subject of *diagnosis*, that they study with the zeal of entomologists to *discriminate minute changes of structure* in the various textures of the human body, but that they *do nothing* to advance the proper business of the physician, the art of healing. Their therapeutics are decried, as showing an ignorance of what has been thought certain in England and in this country; and they themselves are regarded even as indifferent to this branch of science."* (*My Italics.*) This was Armstrong's opinion, who says,—“They (the French) seem as if their only object was to find out the disease without any reference to its relief or cure.” And how find it out? Hear Armstrong again:—“I trust the facts of the French, but I receive them with great caution. They have just burst from the old system of pathology, and seem to me to draw *conclusions too general from a few facts.*” And have I not shown this, most extensively, in regard to M. Louis? What was Laennec's doctrine?—“Pathological anatomy,” he says, “is incontestibly the surest guide of the physician, either in the *recognition* of maladies, or in the *cure* of those which are susceptible of cure.” Andral may be said to personate morbid anatomy as much as M. Louis, and it is from the dead-house that he has exploded inflammation as a disease. Hear Mr. Travers:—“I am desirous,” he says, “to guard myself from the imputation of being in the slightest degree *tainted* by the heresy of the French school, that there is *no such reality as inflammation*; that ‘it is an old-fashioned coin, of which the impression is effaced, and that it ought now to be withdrawn from circulation.’” (*Quoting Andral.*) If the study of diseased actions had gone hand in hand with that of diseased structures, it is impossible that such a doctrine could ever have been advanced. Out of the debris of the dead subject, however accurately inspected, examined and arranged, to attempt a solution of the great problem of living actions, and to build upon such a foundation an edifice of pathology of self-support, is as injurious a fallacy, and scarcely less arrogant or absurd than that of the Cartesian Philosophers, who undertook, out of the depths of their anatomical sagacity—to make a man.” “The effect of morbid anatomy holding the first, and only place in the mind of the medical inquirer, is to substitute effect for cause, the laws of physics for the laws of life, to confound the cause of death with the cause of disease, and in short to obscure by attempts at simplification.”

This is what I have taught in my Essay upon the schools, where I have endeavored to give to morbid anatomy its legitimate importance. That importance will be gathered from the whole Essay, and not from an isolated remark. I have now quoted Mr. Travers for the purpose of exhibiting a perfect portrait of M. Louis's work on the Typhoid Affection. My Essay presents the original, from which the foregoing semblance may be supposed to have been taken in shadow. It is useless to talk of M. Louis's detail of symptoms. The most that he makes of them, is to assume them as indicative of the primary seat of the disease

* Jackson's Preface to Louis on Bloodletting, p. 25.

in a structural lesion of Peyer's glands, as set forth in my Essay at pages 716, 798, &c., whilst my author, as shown at p. 790—797, generally gathered a knowledge of the early symptoms from the patients themselves, or their no less squalid companions.—(See p. 773.) This is fully allowed by Dr. B., who turns aside its obvious application by the remark,—“Suppose Dr. Paine had been the observer,—how would he have learned about the previous history except from the patient and his friends?”—(P. 108.)

Now, in the first place, Dr. Paine objects to taking this class of patients (especially from Parisian hospitals), for the purpose of supplying any useful facts in pathology, and he has given reasons for it. Their constitutions are wretched, their treatment worse, and they are full of organic lesions before they enter the Gallic institutions. As Armstrong, Travers, &c. imply, they are then mostly looked upon as good subjects for the dissecting knife; nor is this ever lost sight of by my author. But, Dr. Bowditch's admission, or interrogatory, amounts to no justification. It leaves the subject just where I had placed it, and shows the absurdity of taking the preliminary symptoms as a true source of knowledge as to the primary seat of the typhoid affection, and, therefore, that my author was fundamentally wrong in putting forth a doctrine which has been at the foundation of his celebrity. It often happened, too, that my author could get no account of the preliminary symptoms.—(P. 790.) Nor should it be forgotten, in allowing my author so much credit for the Hippocratic mode of observing nature, that the typhoid patients were generally severely sick for many days before they took up quarters at La Charité, and were sometimes “*a little delirious*” when supplying the important details for the benefit of science.

Now, although I have made all the foregoing perfectly clear in my Essay, yet, as I can only lay certain parts of it before my reader, I will give him the *quod erat* in words of my author, which, on account of the clearness of my demonstration, I did not think proper to introduce into my work. I endeavored to avoid what might seem supererogatory. Thus, then, my author:—

“Physicians not much conversant with hospitals, or who seldom practise among the laboring classes, will not readily give credit to these remarks; but those differently situated are aware that, whether it be from indifference, or dislike to hospitals, patients *seldom enter until quite late; even when their diseases have been very violent from the beginning.*”*

So much, then, as to tracing the primary seat of the Typhoid Affection to an anatomical lesion of Peyer's glands, and the value of such subjects for important scientific and practical purposes.

What I have said of my author's symptomatology, I have substantiated out of his own works, and this in all parts of my Essay. I remark that,—“Our author's philosophy is necessarily concerned about *symptoms*, so far as they are important in showing that the disease began in the intestinal canal, and were significant of a primary lesion of the glands of Peyer. This is of vital moment to the hypothesis as it

* Louis on Bloodletting, p. 7.—Putnam's Translation and Punctuation. Boston, 1836. *My Italics.*

respects the foundation of the disease, and of the entire superstructure."—(P. 790.) "All the rest reposes mainly upon the *color* of the parts, *red* and *white* being the standards, and as other shades may approach either of the foregoing, they are appropriated accordingly." "Our author's 'Remarks' upon every case consist especially in speculations about the various colors which were observed."—(P. 798.) This is true, and this is the test. His "Remarks," which go to form his pathological conclusions, are almost entirely concerned about the "debris of the body," with the exception of those preliminary symptoms which he got from the patients, their friends, or not at all. Nevertheless, I fully, and repeatedly allowed, in the course of my demonstration, that, "although our author does not take into account the vital signs in making up his principles and generalizations, he has supplied a *memorandum which others may employ*."—(P. 716.) So obvious have I rendered all this, that my able New-York Reviewer, of whom I formerly spoke, manfully takes the broad ground of Morbid Anatomy.

But, suppose it had been otherwise, and my author had taken the vital phenomena, along with the cadaverous decompositions, into his pathological conclusions, what were such subjects worth, and what the value of his generalizations? There is not one case, in my opinion, which was worthy of record, and this opinion I have placed deliberately before the world for what it is worth.

The foregoing subject will continue to be more or less under review, and I may now remark that if anything in the way of proof can bear upon a question, it is, that the fact of my author's having made all the anatomical lesions and all the symptoms, in the typhoid affection, to revolve about the structural lesion of Peyer's glands, proves, beyond controversy, that my author is a "*dead-anatomist*." But more of this hereafter; and, in the mean time let us hear Dr. Lewins as to the anatomical school:—"I have often thought," he says, "that our most eminent pathologists sometimes appear much more desirous of having an opportunity of dissecting the bodies of the dead, than anxious to make any practical application of their post-mortem examinations for the benefit of the living." Nor may it be amiss to quote M. Ribes in my behalf. Thus:—"The time will come when physicians will feel that they have been very justly censured (*qui ils étaient mépris*), for having thought that pathology consisted exclusively in the study of organic degradations, and that they merit the same reproaches which they have bestowed upon others for confiding in the symptoms alone."—(*Anat. Path., &c., Pref., p. vii.*) To whom does Dr. B. suppose that all the foregoing authors refer? This, however, is but that circumstantial proof which consists in universal opinion. I go back, therefore, to the more direct, and will have something more of the internal nature.

Dr. B. offers the alternative of choking me with a "falsehood," or with having read my author in a blundering manner. Not choosing to avail myself of either, it only remains for me, as hitherto, to impale him on both horns of his "dilemma." I protest, however, against all imputation of a proper want of generosity. My reader will yet see that no opportunity has been left for its exercise.

Dr. B. quotes me accurately in the following manner:—"Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure; and when he endeavors to *insinuate* the belief that diarrhœa cannot exist 'without *appreciable lesion* of the intestinal mucous membrane,' he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body."—(P. 695.)

My critic then remarks that,—“Our readers would scarcely believe us if we were merely to state that all this is radically *false*” (p. 91); (*my translator's Italics*); and, after quoting and re-modelling the translation, proceeds to force an induction that I had really affirmed that M. Louis had positively denied, in this place, that “diarrhœa could exist without appreciable lesion of the mucous membrane.” But, although I have stated exactly the fact in this particular instance, I have shown, from my author's work on the typhoid affection, that he does deny the independence of diarrhœa, in that complaint at least, of structural lesion.

I shall now present the whole paragraph which has been quoted by Dr. B., but not according to his new version. I quote from Cowan's translation of the work on Phthisis, “revised and altered” by Dr. Bowditch, and which I formerly employed. Thus:—

“Let us remark, that these copious perspirations indicated disorder in the functions of the skin, as remarkable by its intensity as duration; that this disorder, whether sympathetic or otherwise, was not the less positive, and existed without any sensible change of structure in the organ itself; and that thus a function may be more or less modified during a long period of time, while the organ on which it depends offers no appreciable change of structure. We may also observe, that while facts are wanting to *prove* distinctly that diarrhœa may exist without appreciable lesion of the intestinal mucous membrane, *we may presume this to be the case, from the analogy existing between diarrhœa and more or less profuse perspiration. Of this we cannot be positive, for in our opinion, analogy is only useful to point out fresh subjects for investigation, to lead us to the discovery of facts, but never to supply them. Were it otherwise we might conclude that a thing really existed because it was possible, which is absurd.*”*

The words in Italics embrace the question at issue,—the word “*prove*” being in Italics in the original but not in the new version; which shows the *quo animo* of my translator at the time of each “re-vision and alteration.” In my work occurs the following quotation, preceded by—“He therefore says,” which is left out by Dr. B. as well as the source of the quotation from my work. Thus:—“He therefore says,—

“We may presume that it may exist without, from the analogy existing between diarrhœa and more or less profuse perspiration.” But, ‘*of this we cannot be positive; for in our opinion analogy is only useful to point out fresh subjects for investigation, to lead us to the discovery of facts, &c.*’—(P. 695.)

The first words in Italics are translated anew by Dr. B., “we do

not say that this is proved." Now, taking this extract alone, my statement is fully made out to be perfectly just,—viz., that, "when he endeavors to *insinuate* the belief that diarrhœa cannot exist 'without appreciable lesion of the intestinal mucous membrane,' he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body." But, I might have made my case stronger, in my Essay, by quoting in this place the sentence immediately preceding, in which the word "*prove*" occurs in Italics. And, when the reader regards the little importance which is here attributed to analogy, and that the question is thus turned aside for *fresh investigation*, I should have drawn even a more "rigorous conclusion" than that my author had "*insinuated* the belief," which he carries out in his work on the typhoid fever,—whatever concessions he may have incidentally made in behalf of functional disease (as I remark in my Essay), for the apparent purpose of more effectually establishing the anatomical doctrine by the occasional, but very equivocal, manifestation of impartiality.—(Pp. 703, 704, 708, 716, 766, 790, &c.) Can anything have been fairer, however provoking?

As to the affirmation, that, "Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure," that is of a general nature, as was well understood by Dr. B., and refers to a vast amount of proof which I subsequently offer to that effect. I was employed in the foregoing instance in showing how my author objected to analogy in the hands of others, and how well he could employ it himself; and, in the course of these remarks I stated, what it was a special object of this Essay to illustrate, that, "the whole of our author's stupendous fabric reposes upon morbid anatomy, and upon reasoning from the *dead* 'to the *sick* himself.'"—(P. 698.) It is the proof upon proof which I have offered, embracing a great part of the Essay, at which my critic is annoyed, and therefore hoped to show by the foregoing incidental quotation that M. Louis had been really misrepresented upon this fundamental point. It was one of my constant objects, also, as I have said, to exhibit my author's contradictions, and I have repeated comments upon the subject. It was one of my motives, in allowing by the foregoing quotation that M. Louis left it doubtful whether or not diarrhœa took place "without any appreciable lesion of the intestinal mucous membrane," to contrast the *doubt* with the *certainty* which is expressed in the following quotation. Thus:—

"At this period, in fact [the *commencement*], the *elliptical patches* were, if not in all, at least in *nearly all* the cases, the only part of the canal which was diseased, and consequently, **THE ONLY PART** to which the **DIARRHŒA** could be referred.'"—(P. 758.)

Now, this looks to me, without my other voluminous proof, a good deal like denying that diarrhœa can arise from mere functional disease. This occurs where I am employed in showing by 35 quotations that my author refers all the lesions of structure, and all the symptoms, in the typhoid affection, even those of the brain,* more or less directly to the

* M. Louis, after enforcing his anatomical doctrine, that all the symptoms and structural lesions, in the typhoid affection, should be referred directly to the anatomical lesion of Peyer's glands, but suppos-

altered anatomical structure of Peyer's glands. But, if the foregoing do not satisfy my critic, let him take the following :—

“Although the diarrhœa was *very great*, the mucous membrane of the colon presented *no evident traces* of inflammation, inasmuch as the thickening may be considered as the consequence of the *reaction occasioned by the meteorism*; so that the *alteration of the patches* of the small intestine was doubtless in a great measure *the cause* of the abundance of the alvine discharges.”*—(P. 712.)

Here, then, are laid down several great principles. 1st, that inflammation, or “an appreciable lesion of the intestinal mucous membrane,” is *necessary to diarrhœa*. 2d, that thickening of this membrane, unless it be *red* (which is meant by M. Louis, no matter how a “profuse diarrhœa” may overcome the redness), is not owing to inflammation. 3d, that such a thickening is a consequence of the reaction of the meteorism! at least “in a certain number of cases.” 4th, that, in the typhoid affection there is a specific lesion of structure for the diarrhœa, namely, “a specific alteration” of Peyer's glands.—(See M. Louis, Vol. 2, p. 449.) 5th, that, in the typhoid fever, at least at its commencement, the diarrhœa proceeds from the altered structure of Peyer's glands. 6th, the whole induction, as to the primary seat of the disease (being a fundamental object of M. Louis's treatise), is shown by these extracts, as in numerous other instances, to have been founded on the vague information which he got from the squalid victims of a Parisian hospital, and who, according to my author's own statement, “*seldom enter until it is quite late; even when their diseases have been very violent from the beginning.*”

The structural lesion of Peyer's glands being the pivot about which the whole machinery revolves, I will here present another example, in which my author endeavors to strengthen his induction as to the dependence of the diarrhœa upon that lesion, and in which is involved one of his constant conclusions, either direct or indirect, that there can be no disease without a lesion of structure. It is also an example of the multitudinous instances in which he establishes “important laws” upon the “debris of the body,” and shows how far Dr. B. has correctly informed his readers in contradicting my statement that “pathological anatomy” was mainly the ground of my author's pathological conclusions. I must, however, in the first place, quote my author's premises, which immediately precede the extract in question. Thus :—

“Of all these lesions only one was constantly found, namely, an *alter-*

ing that all may not concur in his views, is peremptory as to the cerebral. Thus :—“We must, moreover, remember that *if the greater part of the symptoms are explained by the state of the organs with which they are connected* [referring to the possible views of others], this is not the case with the *delirium*, which we cannot explain by the apparent state of the brain; [no anatomical doctrine this;] that, *more than any other symptom*, it seems to be dependent upon the small intestine, in the typhoid affection, so that it appears that *its treatment* ought not to differ from that of the *specific alteration* of the small intestine.”—(On Typhoid Fever, Vol. 2, p. 448.) And thus, in my Essay,—“Nor should it be forgotten, that, in nearly all our author's fifty cases, there was either *injection, softening, an accumulation of serum*, or some other unnatural appearance in the brain. ‘The pia mater,’ says our author, ‘was injected in a little less than half the cases’; ‘the medullary substance of the cerebrum was more or less injected in all the cases excepting seven’; and the cerebellum was only ‘more frequently in a healthy state than the cerebrum, and when it was *not so*, it generally was affected in a *similar manner* and in the same cases.’”—(P. 762.) So much for my author's morbid anatomy.

* My Italics, as they generally are when not otherwise indicated.

ation of the *elliptical patches* of the small intestine, to which may be added a morbid change in the mesenteric glands. I have considered it as *inseparable* from the disease we are now studying, and as absolutely forming its anatomical characteristic." "I must conclude *the lesion of the elliptical patches began at the commencement* of the disease. Although the *other lesions* must be considered as *merely accessory or consecutive*, still they commenced often quite soon after *the principal disease*"—[that is to say, the lesion of Peyer's glands]. "The anatomical characteristic of typhoid fever becomes still more manifest by the comparison of *the lesions* previously given, with those presented by individuals who died in consequence of other acute diseases." "But these frequent lesions of the mucous membrane of the alimentary canal and of a variety of other organs, in patients who died of acute diseases of any nature, prove that when an affection of this nature gives rise to a febrile excitement of any duration, the majority of the organs of the body become the seats of more or less serious lesions."—(*M. Louis, Vol. 1, pp. 381, 382.*)

Now follows immediately the extract contained in my Essay:—

"6. 'The *mucous membrane* of the alimentary canal [in the typhoid affection] is not oftener, in fact is less frequently, diseased than some others, the spleen, for example, which was more or less seriously changed in all the cases of the typhoid affection excepting *four*. This is an IMPORTANT LAW, and it may tend, as it appears to me, to *simplify much the STUDY OF PATHOLOGY*. This is what we ought, perhaps, to have discovered A PRIORI'!—(See p. 682, *Sprengell*, and what our author says of "*a priori*" reasoning, p. 680—681.)"

Upon this extract I remarked,—“This is also a striking exemplification of our author's disregard of the symptoms of disease, and that the whole of his pathology consists in lesions of structure; since, in all the fatal cases of the typhoid affection, but 3, there occurred diarrhœa and pains in the abdomen, and they were generally the earliest symptoms.”—(*Com.*, p. 700.)

The reader must now understand, since something is said about *fever* in the foregoing extract, that my author refers this, also, entirely to the structural lesion of Peyer's glands, in the typhoid affection, as he does to the *liver* in yellow fever. Thus, from my Essay:—

“The following generalizations show that our author ascribed the whole febrile action to the structural lesion of the glands of Peyer. Thus:—

“135. 'We must allow that the *febrile excitement* which was observed in the typhoid affection was *as much proportioned to the state of the small intestine*, as that which occurs in *erysipelas* of the face is to the extent of the skin inflamed.'

“136. 'The FUNDAMENTAL ALTERATION, that of the elliptical patches of the small intestine, was more extensive according as the patients died more rapidly, and *the febrile action was generally in proportion to it*.'

“‘*The intensity of the febrile action is itself in proportion to the extent and seriousness of the SPECIFIC lesion of the small intestine*.'

“137. 'Between the SYMPTOMS and LESIONS, of which we are now

treating [glands of Peyer], the relation seems to me to be *not less evident* than that which is observed between *those two orders of facts*, as they take place in other affections; *pneumonia*, for example, &c.

"But, must we suppose this action of the *lungs* and of the *elliptical patches* of the small intestine to be the effect merely of a *sympathy*, which we cannot appreciate, in *their capabilities of producing disease*, or as the *consequence* of a *febrile* excitement of which the *inflammation* of these organs was the *source*. This last supposition seems to me to be the most probable."

"Their great importance [the facts previously given], as it seems to me, consists in this, that they ought to excite doubts in our minds, as *already stated*, in relation to *all febrile affections without a determinate seat*, and which are called *general*, and thus put us in the way of proposing problems which it is of great importance for us to solve.'" Again:—"The number of these febrile affections without any determinate seat or *local lesion* is every day becoming more limited."

"And then follows, in a note, a reference to one of those '*isolated facts*,' which it is our purpose to notice soon; and about which our author hesitates whether he 'shall *prejudge* by it the important question of the nature of fevers.'"

"The fever had its origin, and continuance, in the alteration of the glands of Peyer, even when that alteration was '*latent*.' This refers to a particular case. Thus:—

"Perhaps the reader will ask, if these febrile symptoms, without *diarrhœa*, or *pains* in the abdomen, observed during the first ten days [!], were connected with the commencing *alterations* in the *elliptical patches* of the ileum. The *affirmative* to this question will not appear doubtful, when we remember that the most severe lesions were those of the small intestine."

"138. 'If, then, FROM THE COMMENCEMENT of the affection *until its fatal termination*, ALL the morbid phenomena *were dependent upon*, or connected with THIS SAME lesion, it results that this *can remain LATENT*, during a considerable length of time, or at least *give rise to no characteristic symptom*.'

"Nearly a page is devoted to a minute detail of symptoms which took place antecedently to the arrival of the patient at La Charité, upon which the whole conclusion turns, and an account of which was obtained from the 'ivory-turner' (the patient) himself."—(Pp. 756, 758.) I may also add, that the patient died on the 26th day of disease, and that "*he said* he had been ill during *three weeks*" of that time. This case offers a fair example of my author's philosophy, ratiocination, pathology, origin of his knowledge as to the primary seat of the typhoid affection, &c.

As a question in relation to the *tongue* will come up, I will add here an extract which bears directly upon it, since it presents, also, a summary of my author's views of the controlling agency of Peyer's glands. Thus:—

"59. 'Therefore, all the different conditions of the *tongue* observed during the course of the typhoid fever must be considered as the result

of one and the same cause which is common to them with all secondary lesions which come on, like the former, at various periods of the disease ; —that is to say, the glands of Peyer."

I must break off abruptly for this week.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—I will thank you to omit the publication of the four remaining numbers of my Reply to "H. I. B." During the progress of the preceding, I have apprehended that I might be encroaching too much upon your Journal and upon the rights of other correspondents. This consideration has led me hitherto to request the delay of my Reply, should it interfere with other communications. I perceive by your notices that such communications are accumulating, and I am therefore entirely satisfied that the subject should now rest upon my part, so far as it respects the Journal.

There is one remaining point, however, which requires explanation; all other charges being groundless. I am accused of a misstatement in affirming that "Louis says that the indications to be drawn from the state of the tongue are the 'least important' of any. But we beg the reader to mark well, Louis never stated this, that we can find, in his work on typhus, or in fact in any of his works, so far as we can discover."—(P. 75.)

The words "least important" do not bear the marks of quotation in my work, and it is an induction of my own from extensive premises. It occurs in the following connection and rather incidentally, in Vol. 1, p. 238. Thus.—"They (Galen, &c.) speak of the important indications of the tongue. Baglivi says it supplies the most important, M. Louis the least." In a note I have referred to M. Louis "on Typhoid Fever *passim*." The statement, however, is notoriously true; and the doctrine was the subject of no little comment by reviewers, a few years ago. In my Essay I have presented several extracts from my author to the foregoing effect. The generalizations numbered 57, 58 and 59 are entirely of that import. See, also, M. Louis on Typhoid Fever, Vol. 2, p. 56.

Dr. Bowditch recurs to the foregoing subject at the close of his attack, and imputes to me "a false statement," which he allows to have grown out of an error in his own translation. In consequence of this error on the part of one who had declared his belief that he had translated his author correctly "in every respect," I was betrayed into a partial misapprehension; and this is the only instance in which any departure from strict exactness can be truly alleged against my Essay.

"What," says Dr. B., "can we think of the candor of such a wri-

ter, when he takes the first line of a sentence and makes pages of commentaries upon an isolated assertion," &c. "Our commentator may complain that the sentence was *badly constructed*. We affirm that this is no excuse; for, in the *original*, the *meaning* is perfectly plain, if the punctuation in the translation is faulty."—(P. 107. *My Italics*.)

Dr. Bowditch has given a pretended quotation of six lines, which embraces "the first line" in question; but it is made up of words selected out of more than a page and a half of M. Louis's work, presented as a continuous sentence, and bears the usual marks of a quotation. I shall therefore produce the statement as it occurs in the original translation, observing, also, the punctuation. Thus:—

"1st. *Tongue*. It was almost always natural; that is, it had not any unnatural redness; it was moist and was at times only a little yellowish and whitish in nineteen patients; among whom were all those who died between the eighth and fifteenth days of the disease, and who arrived at the hospital at a sufficiently early period to be examined with care upon this point."—(Vol. 2, p. 55.)

The words quoted, and rather incidentally by me, are "*The tongue was almost always natural*;" to which I add, that this is "contradicted by nearly every one of his exemplifying cases." Now, as the extract stands, the first clause appears as a direct and unqualified affirmation, whilst the details which follow purport a contradiction. These contradictions met me so constantly, that I was here led into an *unimportant* error by the faults in the translation, and, if it can avail its author anything, I take more satisfaction in allowing it, than I have done in the preceding exposition. I had imagined, indeed, from the structure of the sentence, that instead of a semicolon after the word *redness*, there should have been a period. Such, I submit, is the only rational mode of interpreting the paragraph.

But, the error, I repeat, is unimportant, and the clause may be expunged from my Essay, without affecting, in the least, the merits of the question. The fundamental point is set forth in other quotations from my author by which the foregoing is preceded; whilst the affirmation, as quoted by Dr. B. that "the tongue was natural or nearly so in a little less than half the cases," also bears out my commentary that this is "contradicted by nearly every one of his exemplifying cases." This will be seen at once by a reference to my tables, where I underwent the labor of condensing, in their "consecutive order," all the appearances of the tongue in each of the fatal cases. But this was not done with a view to the specific fact in question, but to the general affirmations of my author that little or nothing could be gathered from the appearances of that organ, especially in relation to the stomach. There is not one of the cases, unless it be No. 32 (in which the tongue was "greyish in centre"), that can be regarded as offering in any sense a "natural" condition. My comment upon the statement in question, instead of consisting of "pages," extends only through about a dozen lines.

That a fair opposition would be made to my Essay upon the writings of M. Louis, as well as to many doctrines taught in my work, I was entirely prepared to expect; but, the forbearance with which they have

been treated by the medical press of this country, and the commendations of the work which have wholly surpassed my expectations, render it proper that I should embrace this occasion to express my profound gratitude for so much generosity.

In conclusion, I may suggest the consideration, that what may have borne the aspect of personal severity in the course of my Reply, has been the unavoidable result of my premises, and of the necessary demonstration. The attack was peculiarly personal, and consisted throughout of a series of misrepresentations. It was made by a responsible individual, and through the columns of a distinguished Journal. It related not only to a laborious work which was designed for the public, but to my reputation as a man. I have felt, therefore, that I was at least entitled to a hearing in my own behalf, if no interest existed in relation to my public labors. I have also prepared for the press other works which it is my intention to publish, and it was therefore the more necessary for their present success that I should avert the possible consequences of those misrepresentations which were designed to affect me unjustly as an author. Nor can I permit myself to doubt that every honorable mind, when it shall have regarded my case as its own, will arrive at just conclusions.

The foregoing Reply will be republished, with the remaining refutation, for the purpose of binding it up with my "Commentaries," that its truth may be tested by the work itself, and effectually protect the honor of its author.

M. P.

Dec. 9, 1840.

THE remaining assumptions of Dr. B. involved the necessity of many quotations from my Essay, which I had embraced in the residue of my Reply, and which formed another motive for omitting their publication in the Journal. Charges may be more briefly made, especially where truth is disregarded, than their refutation; and, in this respect the accuser has always an advantage. This I had constantly felt; and now, to avoid any prolixity of detail which may be repulsive to some readers, I have substituted references to my Essay for the extended quotations.

Returning to the question concerning diarrhœa, which was a subject of examination in my fifth number, it will be seen by referring to my Essay, page 759, that I went farther than I was warranted in underrating my author's real belief that diarrhœa depended upon a lesion of structure, and certainly beyond anything that is justified by my quotations as to the dependence of diarrhœa upon a structural lesion in the typhoid affection.

It is obvious from the foregoing reference, that I yielded an advantage to my author's theory of the *primary* seat of the typhoid affection, to which he was not entitled. The reader will have seen the difficulty with my author. The *anatomical* lesion of Peyer's glands was assumed to be the very first in the series of morbid changes, and it was necessary, therefore, to assume, also, that the first symptoms depended upon it. Diarrhœa was almost always one of the first. Hence the object of my argument in the foregoing reference to my Essay; for, whilst I was willing to allow what my author might not be inclined to take, on account of the *glandular* doctrine, I could make out my case that the affection of the *mucous* tissue, *per se*, being primary, the glandular lesion might be only a *secondary* result, and thus sap at the foundation the general doctrine which ascribes all the consecutive lesions to the assumed primary lesion of Peyer's glands.

Again, to show how fully I have represented my author upon this, as on all other questions, I have even presented him as denying a morbid state of the intestinal mucous membrane in *Enteritis*. Thus:—

"141. 'We are led to believe that the mucous membrane of the intestine is but little altered in *Enteritis*, but that it is in a condition which differs perhaps but little from that of the *skin* when it is the seat of copious *perspiration*.'—(P. 764.)

Here, as every where else, we have a practical illustration of my author's conviction of the "paramount importance" of morbid anatomy. I introduced at the close of my fifth number several quotations showing, perhaps with sufficient comprehensiveness, my author's views as to the dependence of the whole array of the typhoid phenomena upon the lesion of Peyer's glands. In my Essay, I went into an elaborate consideration of the subject, and presented a multitude of extracts, gathered from all parts of my author's work, to the foregoing effect; partly because it had been affirmed by my author's friends that "M. Louis did not show nor did he attempt to show, that the disease he described was dependent on the morbid affection of the small intestines," and partly to carry out the intention of the Essay of supplying a practical demonstration of the evils of morbid anatomy when made the great basis of pathology. What, for instance, should be the remedies for *enteritis*, if my author's doctrine be sound, as propounded in the foregoing quotation?

In addition to the extracts in my fifth number, I would now refer to the generalizations marked 120, 123, 124, 126, 128, 129, 130, 131, 132, 133, 134, 142, and many accompanying quotations of the same import, as direct affirmations of the dependence of the phenomena, and the various lesions of organization, upon the structural lesion of Peyer's glands, in the typhoid affection, and, as in most other citations, fully indicative of my author's almost entire dependence upon morbid anatomy for his pathological inductions; about which all correct treatment of disease is necessarily concerned. The whole fabric, in the typhoid affection, turns upon the lesion of Peyer's glands, whilst the subjects themselves were of the worst class, the disease far advanced before their admission (see my tables), the primary symptoms gathered from the purlieus of Paris, the pathological specifications depending mainly upon

shades of color, and the dissections made on an average more than 29 hours after death—often 40, or more. (See tabular view, p. 799.)

The practice of referring the principal attributes of disease or the ulterior results to some particular part which may be found by one anatomist or another most frequently deranged in its structure, has been a prevailing error at former periods, and many philosophers have attempted its exposure. I have quoted from Sprengell, Zimmermann, and others (p. 682, &c.), to show the coincidence, in this respect, of my author's philosophy, not only in regard to the typhoid affection, but the yellow fever, and other diseases. The reader will now comprehend the imputed value of the "900 cases," as examined in my second number, and may decide whether, "in fairness" to science, it was not rather my duty to have placed the whole merits of my author's work upon the morbid anatomy of "the 50 cases of the typhoid affection," than to have admitted other elements which, in reality, formed no part of the ground for the pathological inductions.

And now, in what language shall I speak of Dr. B.'s denial that M. Louis's generalization of the anatomical lesion of Peyer's glands was designed for all climates of the globe, and of his imputation of untruth to me for this intimation? Let the reader turn to the preceding quotations and references, which not only make that lesion indispensable to the typhoid affection, but to all other lesions of structure, and to the symptoms in a general sense. And let him consult my Essay from page 730 to 739 as to the generalizations in respect to remedies, by which my author abolishes bloodletting, blisters, &c., not only in the typhoid affection, but in the gravest internal inflammations,—and from page 700 to 705 as to organic lesions; and he will here find, as throughout my Essay, that my author's conclusions were designed for "laws" of universal application. That my author has avowed a disposition to admit the results of farther observation, that he has acknowledged a diffidence of opinion, and commended a pursuit of "the numerical method," I have fully represented in his own language, and with such comments as seemed to me appropriate. But, all that is thus incidentally said in behalf of farther inquiry affects in no respect the unreserved manner in which he lays down fundamental laws and generalizations, and in opposition to his avowed object of supplying only a record of facts, and in violation of that principle upon which he rejects the learning, the labors, the philosophy, of ages. The allowances which are thus introduced, and in a general manner, only serve to impart greater force to the specific conclusions, whilst we are early admonished to beware of others:—"The reader will pardon us, perhaps, for having insisted so much on the care we have bestowed on the collection of *our* facts, and upon the *distrust* with which part of those *daily* published ought to be received," &c. (On Phthisis, Preface, p. 67.) All the pathological inductions in respect to the great disorganizations in the typhoid affection are absolutely expressed in a fundamental sense, and without reservation.

Again "Dr. Paine states," says Dr. Bowditch, "that the follicular affection is denied to exist by Louis and his followers in genuine typhus. We know not to whom Dr. Paine refers under the title of followers;

but, as it regards Louis, we deny that he ever made such an assertion, and challenge Dr. P. to produce any proof to that effect."—(P. 90.)

The reader will have seen the "proof" in many of the citations and references already made. Not only "the follicular lesion," but as the reader will find, by consulting my Essay, several other "specific" lesions, are appropriated by M. Louis exclusively to "the typhoid affection." But, what are my words, that a charge is thus protruded as if some great injustice had been contrived,—perhaps some quotation mutilated, or some translation condemned, or some fraudulent substitute stealthily advanced? No such thing,—no such "trickery" as our translator has it. My words are incidentally connected with a statement going to invalidate the great attribute of the "typhoid affection"; and that statement was predicated by another writer of the very principle which Dr. B. denies with an assurance which is only equalled by himself. My words are:—

"Dr. Perry states the following as the result of 300 anatomical inspections in the genuine typhus, in which the follicular affection is denied to exist by M. Louis and his followers."—(P. 689.)

It is even impossible for the reader to know whether the imputed affirmation is made by Dr. Perry or by myself. But it was I that made it, as indispensable to the subject matter of proof which I was about to bring from various observers to show that the "follicular lesion" was not "peculiar to the typhoid affection." At the very page from which Dr. B. was elaborating this charge, begin the following remarks, which shew him the true position of my author as to "the typhoid affection" and "the genuine typhus." Thus:—

"In our country," says the British and Foreign Medical Review, "we do not require any arguments to persuade us of the identity of these supposed distinct diseases. The evidence is constantly before our eyes." "We think that the condition of Peyer's and Brunner's glands can, as little as the previous circumstances of contagiousness and period of life, be referred to as any ground for the distinction of typhus and typhoid fever." And thus Roupell:—"The impression on my mind, arising from the correspondence between the leading features as already detailed is, that the two fevers are identical."

Notwithstanding the notoriety of the fact, and the proof which was before the eyes of my critic, when he wrote the foregoing remark, that a vast series of observations have been made in Britain to show the existence of the follicular lesion in "the genuine typhus," and to thus disprove the assumption of M. Louis that it is an anatomical lesion "peculiar" to the typhoid affection, and, although much of M. Louis's real fame rests upon this assumption, let us look at only one or two of the numerous quotations of a concurring import, for the specific "proof" which is demanded. Thus M. Louis:—"We must not consider it merely as peculiar to typhoid affections, but as forming their anatomical characteristic *as much as tubercles do that of phthisis*, whatever may be the cause of their development;" [as much so at Pekin as at Paris].

Now it is notorious that M. Louis did not include the genuine or "contagious" typhus amongst the "typhous affections" of which he was

speaking, and to the former of which Perry, myself, and others were referring. By "typhous affections," M. Louis means affections strictly analogous to "the typhoid disease." The plural also is of very rare occurrence in his work. This is the universal understanding. Take, again, another example,—the able and distinguished paper by Dr. Gerhard, in which he draws an absolute distinction between "typhus fever" and the "typhoid fever." "The typhoid fever," he says, "was placed by this work of Dr. Louis, in the same relation to other fevers that pneumonia holds in reference to the affections of the chest."* The name *dothinen-teritis* was bestowed upon it, to distinguish it from all other fevers, just as *ornithorynchus paradoxicus* was invented to distinguish that animal from all other animals. Indeed, such was the assemblage of imputed aberrations in the "typhoid affection," that my author was embarrassed for a name, and declares in his "advertisement," that—"For a long while he endeavored to find some word, which would express the anatomical characteristics of this disease, without being disagreeable to the ear;" and he actually abandoned it in the end, as a hopeless enterprise. But, we have now got the euphonic word. If we therefore associate all this labor at a distinguishing name, and its original fruitlessness, with all the mysteriousness in which the various structural lesions are involved; that the inflammation and ulceration of Peyer's glands are declared to be of a "specific" nature and "peculiar to the typhoid affection," and that the structural lesions of the alimentary mucous membrane, of the heart, liver, spleen, epiglottis, pharynx, œsophagus, &c., are attributed to a cause "exactly the reverse of inflammation," and are superadded, as well as the rose-colored lenticular spots, the sudamina, the meteorism, the hard patches, perforations of the intestine, as "peculiar" also to the "typhoid affection," amongst acute diseases, we have a grouping of attributes which present that disease as a perfect anomaly in human maladies, and as even falling without the pale of any known laws in the animal economy. (See Comm. pp. 701—709, 712, 719—726.) It is therefore *sui generis*, a perfect *Morbus Paradoxicus*; and were not the name already appropriated to another celebrated complaint, I would call it the *Morbus Gallicus*."

But to put this matter beyond all question, here is my first of numerous quotations to the foregoing effect, viz. :—

"120. 'I have considered it [the alteration of the elliptical patches] as *inseparable* from the disease we are now studying, and as absolutely forming its anatomical characteristic.' " The French—"je l'ai regardé comme *inséparable* de l'existence de l'affection qui nous occupe," is very expressive. This, therefore, *per se*, is equivalent to denying its existence, not only in the "genuine" or "contagious" typhus, but in all other acute diseases.

We will now have a practical illustration, in which M. Louis enforces the peculiarity of the lesion to "the typhoid affection," "qui nous occupe," by comparing it with another disease, as we have just seen of the individuality which he establishes for the typhoid affection, by connecting

* American Journal of Medical and Physical Sciences, Feb. 1837, p. 290.

it with the follicular lesion as he does tubercle with phthisis. Thus, of Obs. 41 :—

"Save in respect to some modifications I shall soon mention, the lesion of the small intestine did not differ in the least from what is observed in subjects who die of the typhoid affection, after having experienced the gravest symptoms. We cannot, therefore, fail of recognizing the character of the disease in this case. This subject had the typhoid affection as much as he has had pneumonia, in whom we find hepatization of the lungs. One case is not less certain than the other" (p. 714); and as good for one climate as for another. And thus again :—

"35 and 36. 'Ulcerations of the œsophagus, like those of the pharynx, seem *peculiar* to patients dying of the affection which we are now examining.'"

"37. 'Therefore, we must consider ulceration, or partial destruction of the epiglottis, as one of the secondary anatomical characteristics of the typhoid affection, as we have already decided that ulcerations of the pharynx and œsophagus are; and that this lesion observed in a patient who should die of any acute affection proves almost to a certainty, *without need of further examination*, that the patient had died of *typhoid fever*.'"

"21. 'Although the hard patches are found in a fourteenth part only of the cases of typhoid fever' [less than four cases], 'it is nevertheless evident, that they are of great value, since, as they appear to be *peculiar* to typhoid fever, they would be sufficient [universally] to enable us to recognize the affection at the first glance.'"

"22 and 23. 'The meteorism and ulcerations of the large intestine are not less important than the hard patches.'"

"29. 'In those cases in which the meteorism is very much marked, we may be able, at the first glance [universally] to *distinguish the Corpse* of a person who had died of *this* disease [the typhoid affection] from that of one who had died of any other acute disease'!"

"26. 'Although in the actual state of science [!] I cannot assign any cause for meteorism, I would, nevertheless, remark that it seemed to me to follow a *LAW* which was analogous to that by which other symptoms are governed.' "There is something *specific* in meteorism'!"

"11. 'The morbid change of the *spleen* has something *special* and *characteristic* about it, in patients who are attacked with the typhoid affection.'"

"30. 'Perforation [of the intestine] in the course of acute diseases, is *peculiar* to the typhoid affection.'"

"31. 'The typhoid disease seems to establish a *marked* predisposition to morbid changes in the mesenteric glands.'"

"32. 'In certain cases we can *determine the period* at which death took place, by means of the mesenteric glands.'"

"40. 'Rose-colored lenticular spots seem to have in this disease something of a *specific* character,' and 'are something *peculiar* to the typhoid affection.'"

"41. 'Since sudamina appeared as frequently in the cases of slight attack as in those which were more severe, must we conclude that this

eruption, like the rose-colored lenticular spots, is something *peculiar* to the typhoid affection? " "

The reader will find in my Essay, various accompanying remarks, and other quotations of a like import, which are too long for repetition, and are clearly unnecessary to decide the question at issue. But, Dr. B. has implied by the foregoing denial, that M. Louis had not restricted the follicular lesion to the typhoid affection, and impugns my veracity in having made such an affirmation, although the foregoing extracts, and others like them, were at the same moment before him. I will therefore convict him out of his own mouth, whilst I show to be true, at the same time, what he is disposed to deny of M. Louis's followers. In his attack (p. 82), he quotes Chomel's opinion (but with whose *facts* I was concerned), to show that I had not correctly represented his (Chomel's) statement as to the anatomical lesion of Peyer's glands. Thus :—

"We conclude from these researches, depending upon numerous observations, agreeing in the most important particulars with those made by Mons. Louis in Paris, and Dr. Bright in England, that the alteration of the intestinal follicles is a condition *wholly peculiar* (tout à fait particulier) to the typhoid affection, the different periods of which we can follow as we can *those of an abscess or a cutaneous exanthema*.—(Leçons, p. 222.)" *The Italics belong to the Critic.*

The reader will have observed that the foregoing quotations are also applicable to other parts of the attack.

I wish not to involve unnecessarily any others in this Reply; but, perhaps I may properly introduce the frank admission of one of my New York Reviewers, and M. Louis's defender, that,—“we fully join with Dr. Paine in thinking that the writings of this author (M. Louis) fully justify the conclusion that he does regard the affection of Peyer's glands *as the disease*, referring the delirium and other remote symptoms *directly* to it.”*

Dr. Bowditch “hastens to another instance of our commentator's unfairness,” and “heartily detests the trickery.”—(Pp. 92, 93.) The subject of complaint is the following passage from my work :—

“Our author has no difficulty with *analogy* where a lesion of structure may embellish the philosophy of disease. Thus :—‘Analogy,’ he says, ‘is in favor of what we advance. For, when hemorrhage occurs in any internal organ, it is almost constantly a symptom of more or less alteration of structure.’ From this assumption he reasons analogically that—‘*hæmoptysis*, with certain exceptions, whenever it occurs, renders tubercles in the lungs *infinitely* probable.’” I then add,—“Here, as we shall see in many other instances, our author makes a demonstrative, though a bad use of analogy, since his *premises being just otherwise*, analogy operates against him.”—(P. 696.)

Now the “trickery” consists in suppressing my commentary, which discloses my object, and represents the whole just as it would have been had I encumbered my pages with the long extract which is prefatory to my quotation, and which is given by Dr. B. to show what I had stated,—viz. that “*his premises were just otherwise*.” Here, too, as in the

* New York Journal of Medicine and Surgery, October, 1840, p. 425.

instance regarding the dependence of diarrhoea upon structural lesion, I was employed exclusively upon the subject of *analogy*, to show, by the habits of my author, how unjust he had been to all other writers, and to science. But, this object is studiously concealed by Dr. B. in both cases, as is also the fact that I had in view one of my author's numerous contradictory statements, and by the concealment of which, and by perverting the import of my subject, Dr. B. represents me as guilty of the very "trickery" about which he was at that moment employed himself. It is exactly of the nature of that which I examined in my second number. So, also, in quoting my extracts from M. Louis upon *one side*, Dr. B. would lead the reader to believe that I had not presented the other,—when, in fact, it was one object of my Essay to exhibit my author's incongruities.

Again, as Dr. B. says,—“We have another specimen of our commentator's unfairness, with either a disposition to lead the reader astray, or great carelessness in quotations.”—(P. 94.) This consists, as Dr. B. affirms, in my having brought two passages from M. Louis's work in juxtaposition, without a proper amplification, to show an inconsistency. The statement, however, which is brought by Dr. B. from a very different part of M. Louis's work, only makes the “inconsistency” more apparent. The subject of complaint relates to my author's doctrine, as quoted in my Essay, that, “it is nearly correct to state that the apparent condition of the brain cannot explain the symptoms of which it is the source, any more than the mucous membrane of the stomach can account for the anorexia and other gastric symptoms in the great majority of cases.”—(*Gen.* 142.) Dr. B. does not like the unqualified manner in which my author lays down this doctrine. That it is exactly so, however, the reader will ascertain by referring to other quotations in which the same doctrine is expressed in the most unqualified manner, and which are marked in my Essay as generalizations 120, 121, 122, 123, 124, 125, 126, 127, 128, 129, 130, with other copious extracts, of a general and specific nature (especially the anatomical at p. 763, &c.). The fact is too obvious for comment, and, as has been stated, is allowed by a defender of my author. Nor could the least qualification of the doctrine arise from what Dr. B. would introduce as to the mucous membrane of the stomach; since his quotation from a remote part of the work shows a violence of gastric symptoms in *two thirds* of the cases, and a “serious alteration of the mucous membrane of the stomach,” in *one third*. And yet in the very face of this quotation Dr. B. adds, “in other words, a proportion of *one half* the cases, but *no corresponding* lesion.” It is also more than once said by my author, that “the mucous membrane of the stomach was, in the *greater proportion* of cases, more or less seriously altered, sometimes softened and thinned, or even destroyed.”—(P. 766.) It is also worthy of remark that my author, when speaking of the structural lesions of the stomach, in the foregoing specification, was employed about the paramount importance of morbid anatomy; so that, it will be seen that Dr. B. has not improved the consistency by introducing an extract which renders my author regardless of the condition of the stomach, and sacrificing every other alteration of

structure to establish the universal dependence of the symptoms upon the lesion of Peyer's glands.

Dr. B. had been quoting from my Essay at page 766 (*Gen.* 142), where the extracts are separated by comments upon each. By turning to that page, the reader will see that I had a higher object in view, in this instance, than of making out an "inconsistency," which is put forth by Dr. B. as the only one. He will next observe that I have here employed no such expression as imputed to me,—“that Louis thinks that the symptoms of disease of the stomach are very obscure,” whilst our translator, in quoting my foregoing extract, suppresses the words, “sometimes softened and thinned, or even destroyed.”

The most culpable act consists in imposing the belief that the whole of this subject had not undergone a full investigation at a preceding part of my Essay, and that I had not represented all my author's antagonizing views for the purpose of proving an “inconsistency.” This, as I have said, is one purpose of my Essay, that I might the more effectually invalidate my author's generalizations, and, also, by thus presenting all the conflicting doctrines, leave no opportunity of imputing to me an “unfairness,”—which is the whole burthen of Dr. B.'s attack. That this was fully done in relation to the stomach, the reader will find by turning to pages 700—701 (*Gen.* 4, 5, 6, 7, 8), 744 (*Gen.* 118, 119). Also, p. 431—433. The school of Broussais, it will be here seen, is in a measure answerable for my author's doctrines. This appears especially in the second paragraph of *Gen.* 7; and, to render that more intelligible, take the following which occurs in my author's work in connection with what I have there quoted. After the words “most obscure,” my author goes on,—

“We can, however, understand why science has made *so little progress* on this point, when we perceive that simple gastritis, or at least what is originally simple gastritis, and which causes death, is of very rare occurrence.” “It is impossible, therefore, under these circumstances, to recognize any symptoms except those of the *most serious lesions*, whilst those dependent upon slighter ones, inevitably escape our notice; but, nevertheless, it is *impossible to know the value of symptoms before we compare them with the condition of the organs.*”—(*M. Louis, Vol. 2, p. 44.*)

The foregoing extract should, also, be considered in connection with what has been said on the subject of “diarrhœa,” and “the paramount importance of morbid anatomy.”* As to the complaint of “bringing together,” how is an author to be appreciated upon questions of the foregoing nature, without a comparison of statements?

Dr. B. objects (p. 95) that I did not quote M. Louis sufficiently as to his induction, that softening of the heart was owing to a *cause* which is thus expressed by my author:—

“14. ‘As I remarked in relation to the softening of the liver, if we knew any cause of disease *exactly the reverse* of inflammation, it would be proper to refer this softening to it.’”

* See my Essay on the Schools, Vol. 2, p. 659.

Now the whole of this conclusion is predicated of mere considerations about color, and the absence of "pus from the walls of the heart." But, Dr. B. thinks that I did not quote my author sufficiently about the shades of color, in this instance,—and this is the gist of the complaint. Nevertheless, I did state every word that is in the least essential to a full understanding of my author, even as it respects the color. But, although color is the main foundation of my author's pathology, and of his generalizations of the structural lesions in the typhoid affection, I might, with entire propriety, have neglected all allusion to it, as it was my object in stating that generalization, to show that my author assigned the *softening of the heart* in the typhoid affection, like that of the spleen and liver, and the ulcerations of the intestines, to "a cause exactly the reverse of inflammation,"—leaving it to the reader to make the best of the pathology, and its appropriate treatment. I felt inclined, however, to let my author have the benefit of his philosophy as to *color*. But, Dr. B. has wholly perverted the plain object I had in view, which was to state an important generalization in pathology, founded upon morbid anatomy; whilst Dr. B. represents it as purporting, especially, to assign the *reasons* for a generalization. Had not Dr. B. also suppressed a part of my extract, and had he said (what he also studiously conceals in other instances), that it was merely offered as the expression of a generalization, the reader would have seen at once what I shall now enable him to understand by presenting the proper extract, which embraces the substantial premises of my author. The words in *Italics* are those which are suppressed by Dr. B. Thus:—

"14. '*The heart had less consistence than natural in 24 subjects.*' 'It was generally livid and purplish on its surface as in its substance. The internal face of the ventricles and auricles was, on the contrary, of a deep-violet-red color.' '*As I remarked in relation to the softening of the liver, if we knew any cause of disease exactly the reverse of inflammation, it would be proper to refer this softening to it.*'"—P. 702.

Now, it is true that Dr. B. has the words which occur after the word *liver*, but disconnected from the extract, and so managed as to purport their absence from my quotation, and even as being extracted by himself from M. Louis, whilst he makes me tacitly refer to them.—(P. 95.) The omission of which Dr. B. complains, on my part, are the words, "It was of an onion-peel color," which precede immediately the foregoing statement beginning with, "it was generally livid," &c. Thus:—"It was of an onion-peel color, and was generally livid and purplish," &c. But, it will be seen that the "onion-peel color" is only a less definite statement than the subsequent, which, being specific and coincident with the former, rendered that superfluous. Dr. B. also complains that I did not add after the words "of a deep violet color," the words—"which color sometimes penetrated beyond the lining membrane, and appeared owing to an *imbibition* of blood, which it resembled more or less in color." My critic farther complains that "where Louis says 'that if we knew any cause of disease,' &c. the commentator says 'our author refers to the absence of pus in the walls of the heart, as a special proof of the foregoing doctrine'; whereas Louis uses this fact, and like-

wise the non-existence of pericarditis in any of the cases [being my critic's inference from the 'sometimes imbibition of blood'], as merely considerations to support, in some measure, his previous arguments [the matter of color], and which to any fair mind are sufficient." "Dr. P. takes what Louis uses as merely supplementary to the main argument [color], and puts it forth as the chief ground-work."

The reader, by comparing my quotation and remarks with the foregoing, will readily perceive the manner in which they have been perverted. I may say, however, that here, as every where else, I have fully represented, in the language of my author, the hues of color as constituting, what appears now to be allowed by Dr. B., "the chief ground-work" of my author's pathology. My commentary adverts to *pus* as only "supplementary" proof, or "a *special* proof;" *color* being the *fundamental* one.

In that part of my article which remains unpublished in the Medical Journal, I went into a long examination of Dr. B.'s statement that "Louis never did propose to decide that a part was inflamed from the existence of redness merely."—P. 95—98.) This I had mainly done to illustrate a doctrine which extends beyond my author, but which is more amply considered in my work. The long extract which Dr. B. has made from my Essay places the foregoing question in its true light, and confirms my author's dependence upon color as a principal element in his pathological considerations, or as Dr. B. has it, in contradiction of himself, forms his "chief ground-work." This is remarkably apparent on almost every page of my author's treatise on the Typhoid Affection, and is variously exemplified in my quotations. "Ulceration," "thickening," "intestinal perforation," "coagulable lymph," "softening," "vast peritoneal adhesions," "glueing together of the intestines," &c. many of which occurred in nearly every case, are sacrificed, as insignificant of inflammation, to the paramount importance of *color*. Illustrating cases of all the foregoing occur in my work. (See Vol. 2, pp. 631—633, 748, 798, 712, 719—721, 749, 319—330.) In the last reference, as also in the very citation which is made by Dr. B. from my Essay, I have quoted my author as having once considered *color* (in his work on Phthisis), as of inferior moment to the greater physical results, but as having abandoned the doctrine in his work on the Typhoid Affection. (See pp. 320, 703.) The school of Broussais had now opened its vast jaws to approaching dislocation, and threatened to swallow up not only my author, but more real giants. My author, too, had a great enterprise to carry, in the glands of Peyer, and to have allowed of inflammation in any other part, especially of *gastritis*, would have been a weapon for his adversary. I considered my author's revolutionary doctrine of immense moment, especially as being sustained by M. Andral and other powerful philosophers, and I endeavored to discharge my task with equal justice to mankind and to my author, and with better fairness than the latter has disposed of his brethren "from Hippocrates to the dawn of his own empire."

Numerous citations occur in my Essay of a direct nature, which establish my author's views upon the question before us. I shall now introduce an example of the whole. Thus:—

"10. 'What deduction shall we make from these facts? Must we admit that the *white* and *red* softening have each their own causes; the one *wholly different from the other*? This question seems to me may now be decided affirmatively, at least in certain cases.'"—(P. 701.)

Take, also, the quotation which occurs exactly at the place where my critic broke off his extract from my Essay. Thus:

"'There is no doubt,' he says, 'that in the first two cases the softening was inflammatory, and that such also was the fact in the third and fourth, *in those spots* where there was a *red* color. But, was this the fact for *those spots* where the mucous membrane was *pale*; and the fifth case, in which throughout the whole extent of the canal there was merely a *gray* color?' " &c.—(P. 704.) The investigation is then carried on under all its aspects, till, remotely from the foregoing, at p. 751, a general summary is presented.

As to the "certain cases," in the foregoing generalization, the reservation is beneath the dignity of science, and is contradicted by the whole of my author's pathology, and reasoning upon color, throughout the work on the typhoid affection. The reader, however, will see, by referring to Dr. B.'s long quotation from my Essay (*Com.* p. 702—704), that I did not affirm what is imputed to me by Dr. B., but that "the inductions rest *chiefly* upon the fact that the foregoing ALTERATIONS of STRUCTURE are *white* in one case and *red* in the other"; whilst perversions of the foregoing nature are interwoven with all parts of the attack, and are even arrayed against the plainest import of accompanying quotations from my work.

It is evident, however, from what Dr. B. has inadvertently admitted as the "ground-work" of my author's pathology, and from introducing his own testimony to sustain the foregoing affirmation, that his convictions are opposed to his professions; since, he represents M. Louis as having once declared in his hearing, "that color alone indicated nothing." Very possibly; but Dr. B. should have recollected that it is not *in the book*; and, moreover, that he had disqualified himself as a witness by affirming that, "we are unwilling to take the assertion of any man."—(P. 75.)

Dr. B. is disposed to think, or rather to imply, that I have not represented M. Louis fairly as to bloodletting in pneumonia. Nevertheless, I have quoted him extensively and fully upon the subject. At pages 730 to 737, there are more than *twenty* distinct extracts respecting this remedy in different acute affections, *eight* of which relate to its application in pneumonia. Other quotations respecting the remedy occur in other parts of the Essay, and under every aspect; but they are entirely too extensive for repetition. The reader, indeed, will be surprised to learn, that my particularity reaches to statements of the effects of bloodletting in pneumonia and the typhoid disease at various stages of the complaints. The remedy is condemned, on the whole, in that desolating inflammatory disease, "the typhoid affection," and is considered about useless in pneumonia, and other scarcely less formidable inflammations. As supplementary to what I have quoted in my Essay, I may add the following, which conveys a just view of my author's opinion of bloodletting in the

three inflammatory affections which form the subjects of investigation in his treatise on that remedy, viz. :—

"From the exposition of facts in this chapter, we infer that bloodletting has had *very little influence* on the progress of pneumonitis, of erysipelas of the face, and of angina tonsillaris, in the cases under my observation; that its influence has not been more evident in the cases bled *copiously and repeatedly*, than in those bled *only once and to a small amount*; that we do not at once arrest inflammations, as is too often fondly imagined; that, in cases where it appears to be otherwise, it is undoubtedly owing, either to an error in diagnosis, or to the fact that the bloodletting was practised at an advanced period of the disease, when it had nearly run its course."—(*On Bloodletting*, p. 22.)

I wish not to resort to any foreign help upon this question, as the proof is ample in my Essay, as derived from my author's works. Nevertheless, I will say, that his efforts to exclude bloodletting from the practice of medicine has been proverbially known; and the Medico-Chirurgical Review, in referring to the subject, speaks of it in that manner, and as being commended by his followers. Thus :—

"It would seem, however, that he (M. Louis) has not abandoned the practice (bloodletting), although we were assured that this conclusion was drawn from the most accurate arithmetical and statistical data! How are we to place *any confidence* in the practical directions of *such a writer*, however high his attainments as a morbid anatomist may be? And yet, some of our most popular authors of the present day attempt to laud him as another *Sydenham* or *Morgagni*!"—(*Medico-Chirurgical Review*, Oct. 1839, p. 543.)*

Although Dr. B. is not satisfied with my "fairness," he complains of my prolixity. But this is owing to the profusion of my quotations from the works under review. Besides exhibiting my author's doctrines in their various aspects, I performed the unexampled task of placing a marginal reference to nearly every one of the quotations. Besides 32 extracts from various works by my author, expressive of his sentiments of the profession, there are, in this Essay, 470 distinct marginal references to his works. The text also embraces many other analytical notices with references which do not appear in the margin. Thus, at pages 732—733, there are 21, and at page 799 there are 54. There is also a tabular view of six pages, and of great labor, in which I have analyzed the whole of the 54 fatal cases, having affixed to each case the duration of the disease before admission, how long diarrhœa had existed, cathar-

* In my Essay I have the following quotation from M. Louis on Bloodletting.

"Need I repeat, that an excellent mode of arresting diseases is to confound them, or at least to make no distinction in the periods at which such and such remedies were employed, as I have pointed out in the preceding chapter."

This has been turned off, in another quarter, "as an ironical hit at Bouillaud and others." There is no such reference intended. It is the doctrine of my author, as fully shown by the last clause of the quotation; and, if the reader will turn to the "preceding chapter," he will there find it fully carried out; nor had there been the most distant allusion made to Bouillaud, or to any other observer. In the same journal one of my quotations from M. Louis is repeated by the Reviewer, and an objection raised which will be found to depend entirely upon the commentator's neglect of an important fact, and of his omission of my marks of quotation, both in the extract and marginal references. The extract is the first of the series at page 693. It is an important one, and is obviously designed in this place as a simple reference to two principal doctrines of my author. But, at page 681—682, and again at page 706 (*Gen.* 17 and 18), the quotation is repeated in full, and according to its individual aspects. I notice the mistake from the convenience of the opportunity.

tics, enemas, bloodletting, leeches, other remedies, diet, appearances of the tongue in the order of occurrence, and how long the patient survived the treatment. I have also brought together the whole fabric of 151 great "laws" and generalizations in pathology, and therapeutics, duly arranged according to their affinities and repulsions, illustrated by the ratiocination of my author, with a copious detail of the qualifications of the subjects upon whom the observations were made; having thus, and in various other analogous respects, carried out the requisitions of the "numerical method." The friends of this school, therefore, and those of the anatomical, should be the last to complain.